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PURITANISM, PIETISM, AND SCIENCE

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IN his prolegomena to a cultural sociology Alfred Weber has discriminated between the processes of society, culture, and civilization.[1] Since his primary interest lay in differentiating these categories of sociological phenomena, Weber in large measure ignored their specific inter-relationships; a field of study which is fundamental for the sociologist. It is precisely this interaction between certain elements of culture and civilization, with especial reference to seventeenth-century England, which constitutes the object-matter of the present essay.

I

The first section of this paper outlines the Puritan value-complex in so far as it was related to the notable increase of interest in science during the latter part of the seventeenth century, while the second section presents the relevant empirical materials concerning the differential cultivation of natural science by Protestants and other religious affiliates.

It is the thesis of this study that the Puritan ethic, as an ideal-typical expression of the value-attitudes basic to ascetic Protestantism generally, so canalized the interests of seventeenth-century Englishmen as to constitute one important *element* in the enhanced cultivation of science. The deep-

[1] Alfred Weber, "Prinzipielles zur Kultursoziologie: Gesellschaftsprozess, Zivilisationsprozess und Kulturbewegung," *Archiv für Sozialwissenschaft und Sozialpolitik*, xlvii (1920), 1–49. See the similar classification by R. M. MacIver, *Society: Its Structure and Changes* (New York, 1931), chap. xii; and the discussion of these studies by Morris Ginsberg, *Sociology* (London, 1934), 45–52.

rooted religious *interests*[1] of the day demanded in their forceful implications the systematic, rational, and empirical study of Nature for the glorification of God in His works and the control of the corrupt world.

It is possible to determine the extent to which the values of the Puritan ethic stimulated interest in science by surveying the attitudes of the contemporary scientists. Of course, there is a marked possibility that in studying the avowed motives of scientists we are dealing with rationalizations, with "derivations," rather than with accurate statements of the "actual motives." In such instances, although they may refer to isolated specific cases, the value of our study is by no means vitiated, for these conceivable rationalizations themselves are evidence (Weber's *Erkenntnismitteln*) of the motives which were regarded as socially acceptable, since, as Kenneth Burke puts it, "a terminology of motives is moulded to fit our general orientation as to purposes, instrumentalities, the good life, etc."

Robert Boyle was one of the scientists who attempted explicitly to link the place of science in social life with other cultural values, particularly in his *Usefulness of Experimental Natural Philosophy*. Such attempts were likewise made by John Ray, whose work in every branch of natural history was path-breaking and who was characterized by Haller as the greatest botanist in the history of man; Francis Willughby, who was perhaps as eminent in zoology as was Ray in botany; John Wilkins, one of the leading spirits in the "invisible College" which developed into the Royal Society;

[1] "Nicht die ethische Theorie theologischer Kompendien, die nur als ein (unter Umständen allerdings wichtiges) Erkenntnismittel dient, sondern die in den psychologischen und pragmatischen Zusammenhängen der Religionen gegründeten praktischen Antriebe zum Handeln sind das, was in Betracht kommt [unter 'Wirtschaftsethik' einer Religion]." Max Weber, *Gesammelte Aufsätze zur Religionssoziologie* (Tübingen, 1920), i, 238. As Weber justly indicates, one freely recognizes the fact that religion is but *one* element in the determination of the religious ethic, but none the less it is at present an insuperable, and for our purposes, unnecessary task to determine *all* the component elements of this ethic. That problem awaits further analysis and falls without the scope of this study.

Oughtred, Wallis, and others. For additional evidence we can turn to the scientific body which, arising about the middle of the century, provoked and stimulated scientific advance more than any other immediate agency: the Royal Society. In this instance we are particularly fortunate in possessing a contemporary account written under the constant supervision of the members of the Society so that it might be representative of their views as to the motives and aims of that association. This is Thomas Sprat's widely read *History of the Royal-Society of London*, published in 1667, after it had been examined by Wilkins and other representatives of the Society.[1]

Even a cursory examination of these writings suffices to disclose one outstanding fact: certain elements of the Protestant ethic had pervaded the realm of scientific endeavour and had left their indelible stamp upon the attitudes of scientists toward their work. Discussions of the why and wherefore of science bore a point-to-point correlation with the Puritan teachings on the same subject. Such a dominant force as was religion in those days was not, and perhaps could not be compartmentalized and delimited. Thus, in Boyle's highly commended apologia for science it is maintained that the study of Nature is to the greater glory of God and the Good of Man.[2] This is the motif which recurs in constant measure. The juxtaposition of the spiritual and the material is characteristic. This culture rested securely on a substratum of utilitarian norms which constituted the measuring-rod of the desirability of various activities. The definition

[1] *Cf.* C. L. Sonnichsen, *The Life and Works of Thomas Sprat* (Harvard University, unpublished doctoral dissertation, 1931), 131 ff., where substantial evidence of the fact that the *History* is representative of the views of the Society is presented. It is of further interest that the statements in Sprat's book concerning the aims of the Society bear a distinct similarity on every score to Boyle's characterizations of the motives and aims of scientists in general. This similarity is evidence of the dominance of the ethos which included these attitudes.

[2] Robert Boyle, *Some Considerations touching the Usefulness of Experimental Natural Philosophy* (Oxford, 1664), 22 ff. See, also, the letters of William Oughtred in *Correspondence of Scientific Men of the Seventeenth Century*, edited by S. J. Rigaud (Oxford, 1841), xxxiv, *et passim*; or the letters of John Ray in the *Correspondence of John Ray*, edited by Edwin Lankester (London, 1848), 389, 395, 402, *et passim*.

of action designed for the greater glory of God was tenuous and vague, but utilitarian standards could easily be applied.

Earlier in the century, this keynote had been sounded in the resonant eloquence of that " veritable apostle of the learned societies," Francis Bacon. Himself the initiator of no scientific discoveries; unable to appreciate the importance of his great contemporaries, Gilbert, Kepler, and Galileo; naïvely believing in the possibility of a scientific method which " places all wits and understandings nearly on a level "; a radical empiricist holding mathematics to be of no use in science; he was, nevertheless, highly successful as one of the principal protagonists of a positive social evaluation of science and of the disclaim of sterile scholasticism. As one would expect from the son of a " learned, eloquent, and religious woman, full of puritanic fervour " who was admittedly influenced by his mother's attitudes, he speaks in the *Advancement of Learning* of the true end of scientific activity as the " glory of the Creator and the relief of man's estate." Since, as is quite clear from many official and private documents, the Baconian teachings constituted the basic principles on which the Royal Society was patterned, it is not anomalous that in the charter of the Society the same sentiment is expressed.

Boyle, in his last will and testament, echoes the same attitude, petitioning the Fellows of the Society in this wise: " Wishing them also a happy success in their laudable attempts, to discover the true Nature of the Works of God; and praying that they and all other Searchers into Physical Truths, may Cordially refer their Attainments to the Glory of the Great Author of Nature, and to the Comfort of Mankind."[1] John Wilkins proclaimed the experimental study of Nature to be a most effective means of begetting in men a veneration for God.[2] Francis Willughby was prevailed upon to publish his

[1] Quoted by Gilbert, Lord Bishop of Sarum, *A Sermon preached at the Funeral of the Hon. Robert Boyle* (London, 1692), 25.

[2] *Principles and Duties of Natural Religion* (London, 1710—sixth edition), 236, *et passim.*

works—which he had deemed unworthy of publication—only when Ray insisted that it was a means of glorifying God.[1] Ray's *Wisdom of God*, which was so well received that five large editions were issued in some twenty years, is a panegyric of those who glorify Him by studying His works.[2]

To a modern, comparatively untouched by religious forces, and noting the almost complete separation, if not opposition, between science and religion to-day, the recurrence of these pious phrases is apt to signify merely customary usage, and nothing of deep-rooted motivating convictions. To him these excerpta would seem to be a case of *qui nimium probat nihil probat*. But such an interpretation is possible only if one neglects to translate oneself within the framework of seventeenth-century values. Surely such a man as Boyle, who spent considerable sums to have the Bible translated into foreign tongues, was not simply rendering lip service. As G. N. Clark very properly notes in this connexion:

> "There is . . . always a difficulty in estimating the degree to which what we call religion enters into anything which was said in the seventeenth century in religious language. It is not solved by discounting all theological terms and treating them merely as common form. On the contrary, it is more often necessary to remind ourselves that these words were then seldom used without their accompaniment of meaning, and that their use did generally imply a heightened intensity of feeling."[3]

The second dominant tenet in the Puritan ethos designated social welfare, the good of the many, as a goal ever to be held in mind. Here again the contemporary scientists adopted an objective prescribed by the current values. Science was to be fostered and nurtured as leading to the domination of Nature through the facilitation of technologic invention. The Royal Society, we are told by its worthy

[1] *Memorials of John Ray*, 14 f.
[2] *Wisdom of God* (London, 1691), 126–129, *et passim*.
[3] G. N. Clark, *The Seventeenth Century* (Oxford, 1929), 323.

historian, " does not intend to stop at some particular benefit, but goes to the root of all noble inventions."[1] But those experiments which do not bring with them immediate gain are not to be contemned, for as the noble Bacon has declared, experiments of Light ultimately conduce to a whole troop of inventions useful to the life and state of man. This power of science to better the material condition of man, he continues, is, apart from its purely mundane value, a good in the light of the Evangelical Doctrine of Salvation by Jesus Christ.

And so on through the principles of Puritanism there was the same point-to-point correlation between them and the attributes, goals, and results of science. Such was the contention of the protagonists of science at that time. Puritanism simply made articulate the basic values of the time. If Puritanism demands systematic, methodic labour, constant diligence in one's calling, what, asks Sprat, more active and industrious and systematic than the Art of Experiment, which " can never be finish'd by the perpetual labours of any one man, nay, scarce by the successive force of the greatest Assembly?"[2] Here is employment enough for the most indefatigable industry, since even those hidden treasures of Nature which are farthest from view may be uncovered by pains and patience.[3]

Does the Puritan eschew idleness because it conduces to sinful thoughts (or interferes with the pursuit of one's vocation)? " What room can there be for low, and little things in a mind so usefully and successfully employ'd [as in natural philosophy]?"[4] Are plays and play-books pernicious and flesh-pleasing (and subversive of more serious pursuits)?[5]

[1] Thomas Sprat, *History of the Royal-Society*, 78–79.

[2] *Ibid.*, 341–2.

[3] Ray, *Wisdom of God*, 125.

[4] Sprat, *op. cit.*, 344–5.

[5] Richard Baxter, *Christian Directory* (London, 1825—first published in 1664), i, 152; ii, 167. *Cf.* Robert Barclay, the Quaker apologist, who specifically suggests " geometrical and mathematical experiments " as innocent divertisements to be sought instead of pernicious plays. *An Apology for the True Christian Divinity* (Phila., 1805—first written in 1675), 554–5.

Then it is the "fittest season for Experiments to arise, to teach us a Wisdome, which springs from the depths of Knowledge, to shake off the shadows, and to scatter the mists [of the spiritual distractions brought on by the Theatre]."[1] And finally, is a life of earnest activity within the world to be preferred to monastic asceticism? Then recognize the fact that the study of natural philosophy "fits us not so well for the secrecy of a Closet: It makes us serviceable to the World."[2] In short, science embodies two highly prized values: utilitarianism and empiricism.

In a sense this explicit coincidence between Puritan tenets and the qualities of science as a calling is casuistry. It is an express attempt to fit the scientist *qua* pious layman into the framework of the prevailing social values. It is a bid for religious and social sanction, since both the constitutional position and the personal authority of the clergy were much more important then than now. But this is not the entire explanation. The justificatory efforts of Sprat, Wilkins, Boyle, or Ray do not simply represent opportunistic obsequiousness, but rather an earnest attempt to justify the ways of science to God. The Reformation had transferred the burden of individual salvation from the Church to the individual, and it is this "overwhelming and crushing sense of the responsibility for his own soul" which explains the acute religious interest. If science were not demonstrably a "lawful" and desirable calling, it dare not claim the attention of those who felt themselves "ever in the Great Taskmaster's eye." It is to this intensity of feeling that such apologias were due.

The exaltation of the faculty of reason in the Puritan ethos —based partly on the conception of rationality as a curbing

[1] Sprat, *op. cit.*, 362.

[2] *Ibid.*, 365–6. Sprat perspicaciously suggests that monastic asceticism induced by religious scruples was partially responsible for the lack of empiricism of the Schoolmen. "But what sorry kinds of Philosophy must the Schoolmen needs produce, when it was part of their Religion, to separate themselves, as much as they could, from the converse of mankind? When they were so farr from being able to discover the secrets of Nature, that they scarce had opportunity to behold enough of its common works." *Ibid.*, 19.

device of the passions—inevitably led to a sympathetic attitude toward those activities which demand the constant application of rigorous reasoning. But again, in contrast to medieval rationalism, reason is deemed subservient and auxiliary to empiricism. Sprat is quick to indicate the pre-eminent adequacy of science in this respect.[1] It is on this point probably that Puritanism and the scientific temper are in most salient agreement, for the combination of *rationalism and empiricism* which is so pronounced in the Puritan ethic forms the essence of the spirit of modern science. Puritanism was suffused with the rationalism of neo-Platonism, derived largely through an appropriate modification of Augustine's teachings. But it did not stop there. Associated with the designated necessity of dealing successfully with the practical affairs of life within this world—a derivation from the peculiar twist afforded largely by the Calvinist doctrine of predestination and *certitudo salutis* through successful worldly activity—was an emphasis upon empiricism. These two currents brought to convergence through the ineluctable logic of an inherently consistent system of values were so associated with the other values of the time as to prepare the way for the acceptance of a similar coalescence in natural science.

Empiricism and rationalism were canonized, beatified, so to speak. It may very well be that the Puritan ethos did not directly influence the method of science and that this was simply a parallel development in the internal history of science, but it is evident that through the psychological compulsion toward certain modes of thought and conduct this value-complex made an empirically-founded science commendable rather than, as in the medieval period, reprehensible or at best acceptable on sufferance. This could not but

[1] Sprat, *op. cit.*, 361. Baxter in a fashion representative of the Puritans decried the invasion of " enthusiasm " into religion. Reason must " maintain its authority in the command and government of your thoughts. *CD.*, ii, 199. In like spirit, those who at Wilkins' lodgings laid the foundation of the Royal Society " were invincibly arm'd against all the inchantments of Enthusiasm," Sprat, *op. cit.*, 53.

have directed some talents into scientific fields which otherwise would have engaged in more highly esteemed professions. The fact that science to-day is largely if not completely divorced from religious sanctions is itself of interest as an example of the process of secularization.

The beginnings of such secularization, faintly perceptible in the latter Middle Ages, are manifest in the Puritan ethos. It was in this system of values that reason and experience were first markedly considered as independent means of ascertaining even religious truths. Faith which is unquestioning and not "rationally weighed," says Baxter, is not faith, but a dream or fancy or opinion. In effect, this grants to science a power which may ultimately limit that of theology.

Thus, once these processes are clearly understood, it is not surprising or inconsistent that Luther particularly, and Melanchthon less strongly, execrated the cosmology of Copernicus and that Calvin frowned upon the acceptance of many scientific discoveries of his day, while the religious ethic which stemmed from these leaders invited the pursuit of natural science.[1] In so far as the attitudes of the theologians dominate over the, in effect, subversive religious ethic,—as did Calvin's authority in Geneva until the early eighteenth century—science may be greatly impeded. But with the relaxation of this hostile influence and with the development of an ethic, stemming from it and yet differing significantly, science takes on a new life, as was indeed the case in Geneva.

[1] On the basis of this analysis, it is surprising to note the statement *accredited* to Max Weber that the opposition of the Reformers is sufficient reason for not coupling Protestantism with scientific interests. See *Wirtschaftsgeschichte* (München, 1924), 314. This remark is especially unanticipated since it does not at all accord with Weber's discussion of the same point in his other works. *Cf. Religionssoziologie*, i, 141, 564; *Wissenschaft als Beruf* (München, 1921), 19–20. The probable explanation is that the first is not Weber's statement, since the *Wirtschaftsgeschichte* was compiled from classroom notes by two of his students who may have neglected to make the requisite distinctions. It is unlikely that Weber would have made the elementary error of confusing the Reformers' opposition to certain scientific discoveries with the unforeseen consequences of the Protestant ethic, particularly since he expressly warns against the failure to make such discriminations in his *Religionssoziologie*.

Perhaps the most directly effective element of the Protestant ethic for the sanction of natural science was that which held that the study of nature enables a fuller appreciation of His works and thus leads us to admire the Power, Wisdom, and Goodness of God manifested in His creation. Though this conception was not unknown to medieval thought, the consequences deduced from it were entirely different. Thus Arnaldus of Villanova, in studying the products of the Divine Workshop, adheres strictly to the medieval ideal of determining properties of phenomena from *tables* (in which all combinations are set forth according to the canons of logic). But in the seventeenth century, the contemporary emphasis upon empiricism led to investigating nature primarily through observation.[1] This difference in interpretation of substantially the same doctrine can only be understood in the light of the different values permeating the two cultures.

For a Barrow, Boyle, or Wilkins, a Ray or Grew, science found its rationale in the end and all of existence: glorification of God. Thus, from Boyle:[2]

"... God loving, as He deserves, to be honour'd in all our Faculties, and consequently to be glorified and acknowledg'd by the acts of Reason, as well as by those of Faith, there must be sure a great Disparity betwixt that general, confus'd and lazy Idea we commonly have of his Power and Wisdom, and the distinct, rational and affecting notions of those Attributes which are form'd by an attentive Inspection of those Creatures in which they are most legible, and which were made chiefly for that very end."

Ray carries this conception to its logical conclusion, for if Nature is the manifestation of His power, then nothing in

[1] Walter Pagel, "Religious motives in the medical biology of the seventeenth century," *Bulletin of the Institute of the History of Medicine*, III (1935), 214–15.

[2] *Usefulness of Experimental Natural Philosophy*, 53; *cf.* Ray, *Wisdom of God*, 132; Wilkins, *Natural Religion*, 236 ff; Isaac Barrow, *Opuscula*, iv, 88 ff; Nehemiah Grew, *Cosmologia sacra* (London, 1701), who points out that "God is the original End," and that "we are *bound* to study His works."

Nature is too mean for scientific study.[1] The universe and the insect, the macrocosm and microcosm alike, are indications of " divine Reason, running like a Golden Vein, through the whole leaden Mine of Brutal Nature."

Up to this point we have been concerned in the main with the directly felt sanction of science through Puritan values. While this was of great influence, there was another type of relationship which, subtle and difficult of apprehension though it be, was perhaps of paramount significance. It has to do with the preparation of a set of largely implicit assumptions which made for the ready acceptance of the scientific temper characteristic of the seventeenth and subsequent centuries. It is not simply that Protestantism implicitly involved free inquiry, *libre examen*, or decried monastic asceticism. These are important but not exhaustive.

It has become manifest that in each age there is a system of science which rests upon a set of assumptions, usually implicit and seldom questioned by the scientists of the time.[2] The *basic* assumption in modern science " is a widespread, instinctive conviction in the existence of an *Order of Things*, and, in particular, of an Order of Nature."[3] This belief, this faith, for at least since Hume it must be recognized as such, is simply " impervious to the demand for a consistent rationality." In the systems of scientific thought of Galileo, Newton, and of their successors, the testimony of experiment is the ultimate criterion of truth, but the very notion of experiment is ruled out without the prior assumption that Nature constitutes an intelligible order, so that when appropriate questions are asked, she will answer, so to speak.

[1] Ray, *Wisdom of God*, 130 ff. Max Weber quotes Swammerdam as saying: " ich bringe Ihnen hier den Nachweis der Vorsehung Gottes in der Anatomie einer Laus." *Wissenschaft als Beruf*, 19.

[2] A. E. Heath, in *Isaac Newton* : *A Memorial Volume*, ed. by W. J. Greenstreet (London, 1927), 133 ff; E. A. Burtt, *The Metaphysical Foundations of Modern Physical Science* (London 1925).

[3] A. N. Whitehead, *Science and the Modern World* (New York, 1931), 5 ff.

Hence this assumption is final and absolute.[1] As Professor Whitehead indicated, this " Faith in the possibility of science, generated antecedently to the development of modern scientific theory, is an unconscious derivative from medieval theology." But this conviction, prerequisite of modern science though it be, was not sufficient to induce its development. What was needed was a constant interest in searching for this order in nature in an empirico-rational fashion, that is, an *active* interest in this world and its occurrences plus a specific frame of mind. With Protestantism, religion provided this interest: it actually imposed obligations of intense concentration upon secular activity with an emphasis upon experience and reason as bases for action and belief.

Even the Bible as final and complete authority was subject to the interpretation of the individual upon these bases. The similarity in approach and intellectual attitude of this system to that of the contemporary science is of more than passing interest. It could not but mould an attitude of looking at the world of sensuous phenomena which was highly conducive to the willing acceptance, and indeed, preparation for, the same attitude in science. That the similarity is deep-rooted and not superficial may be gathered from the following comment upon Calvin's theology.[2]

" Die Gedanken werden objektiviert und zu einem objektiven Lehrsystem aufgebaut und abgerundet. Es bekommt geradezu ein naturwissenschaftliches Gepräge; es ist klar, leicht fassbar und formulierbar, wie alles, was der äusseren Welt angehört, klarer zu gestalten ist als das, was im Tiefsten sich abspielt."

The conviction in immutable law is as pronounced in the theory of predestination as in scientific investigation: " the

[1] *Cf.* E. A. Burtt in *Isaac Newton: A Memorial Volume*, 139. For the classic exposition of this scientific faith, see Newton's " Rules of Reasoning in Philosophy," in his *Principia* (London, 1729 ed.), ii, 160 ff.

[2] Hermann Weber, *Die Theologie Calvins* (Berlin, 1930), 23.

immutable law is there and must be acknowledged."[1] The similarity between this conception and the scientific assumptions is clearly drawn by Hermann Weber:[2]

". . . die Lehre von der Prädestination in ihrem tiefsten Kerne getroffen zu sein, wenn mann sie als Faktum im Sinne eines naturwissenschaftlichen Faktums begreift, nur dass das oberste Prinzip, das auch jedem naturwissenschaftlichen Erscheinungskomplex zugrunde liegt, die im tiefsten erlebte gloria dei ist."

The cultural environment was permeated with this attitude toward natural phenomena which was derived from both science and religion and which enhanced the continued prevalence of conceptions characteristic of the new science.

There remains a supremely important part of this study to be completed. It is not sufficient verification of our hypothesis that the cultural attitudes induced by the Protestant ethic were favourable to science. Nor, yet again, that the consciously felt motivation of many eminent scientists was provided by this ethic. Nor, still further, that the cast of thought which is characteristic of modern science, namely, the combination of empiricism and rationalism and the faith in the validity of one basic postulate, an apprehensible order in Nature, bears an other than fortuitous congruency with the values involved in Protestantism. All this can but provide formidable evidence of a certain probability of the connexion we are arguing. The most significant test of our hypothesis, the *experimentum crucis*, as it were, is to be found in the confrontation of the results *deduced* from our theory with relevant empirical data. If the Protestant ethic involved an attitudinal set favourable to science and technology in so many ways, then we should find amongst Protestants a greater propensity for these fields of endeavour than one would expect

[1] Hermann Weber, *Die Theologie Calvins* (Berlin, 1930), 29. The significance of the doctrine of God's foreknowledge for the re-enforcement of the belief in natural law is remarked by H. T. Buckle, *History of Civilization in England* (New York, 1925), 482.

[2] *Ibid.*, 31.

simply on the basis of their representation in the total population. Moreover, if, as has been frequently suggested,[1] the impression made by this ethic has lasted long after much of its theological basis has been largely disavowed, then even in periods subsequent to the seventeenth century, this connexion of Protestantism and science should subsist to some degree. The following section, then, will be devoted to an experimental test of our hypothesis.

II

In the beginnings of the Royal Society there is found a closely wrought nexus between science and society. The Society itself arose from an antecedent interest in science and the subsequent activities of its members provided an appreciable impetus to further scientific advance. The inception of this group is found in the occasional meetings of devotees of science in 1645 and following. Among the leading spirits were John Wilkins, John Wallis, and soon afterwards Robert Boyle and Sir William Petty, upon all of whom religious forces seem to have had a singularly strong influence.

Wilkins, later an Anglican bishop, was raised at the home of his maternal grandfather, John Dod, an outstanding Nonconformist theologian, and " his early education had given him a strong bias toward Puritanical principles." [2] Wilkins' influence as Warden of Wadham College was profound; under it came Ward, Rooke, Wren, Sprat, and Walter Pope (his half-brother), all of whom were original members of the Royal Society.[3] John Wallis, to whose *Arithmetica Infinitorum* Newton was avowedly indebted for many of his leading

[1] As Troeltsch puts it: " The present-day world does not live by logical consistency, any more than any other; spiritual forces can exercise a dominant influence even where they are avowedly repudiated." *Die Bedeutung des Protestantismus für die Entstehung der modernen Welt* (München, 1911), 22; cf. Georgia Harkness, *John Calvin : The Man and His Ethics* (New York 1931), 7 ff.

[2] *Memorials of John Ray*, 18–19; P. A. W. Henderson, *The Life and Times of John Wilkins* (London, 1910), 36. Moreover, after Wilkins took holy orders, he became chaplain to Lord Viscount Say and Seale, a resolute and effective Puritan.

[3] Henderson, *op. cit.*, 72–3.

mathematical conceptions, was a clergyman with strong leanings toward the Puritan principles. The piety of Boyle has already been remarked; the only reason he did not take holy orders, as he said, was because of the " absence of an inner call." [1]

Theodore Haak, the German virtuoso who played so prominent a part in the formation of the Royal Society, was a pronounced Calvinist. Denis Papin, who during his prolonged stay in England contributed notably to science and technology, was a French Calvinist compelled to leave his country to avoid religious persecution. Thomas Sydenham, sometimes called " the English Hippocrates," was an ardent Puritan who fought as one of Cromwell's men. Sir William Petty was a latitudinarian; he had been a follower of Cromwell, and in his writings he evinced clearly the influences of Puritanism. Of Sir Robert Moray, described by Huyghens as the " Soul of the Royal Society," it could be said that " religion was the mainspring of his life, and amidst courts and camps he spent many hours a day in devotion." [2]

It is hardly a fortuitous circumstance that the leading figures of this nuclear group of the Royal Society were divines or eminently religious men, though it is not quite accurate to maintain, as did Dr. Richardson, that the beginnings of the Society were amongst a small group of learned men in which Puritan *divines* predominated.[3] But it is quite clearly true that the originative spirits of the Society were markedly influenced by Puritan conceptions.

Dean Dorothy Stimson, in a recently published paper, has

[1] *Dictionary of National Biography*, ii, 1028. This reason, effective also for Sir Samuel Morland's turning to mathematics rather than to the ministry, is an example of the direct working of the Protestant ethic which, as exposited by Baxter, for example, held that only those who felt an " inner call " should enter the clergy, and that others could better serve society by adopting other accredited secular activities. On Morland, see the " Autobiography of Sir Samuel Morland," in J. O. Halliwell-Phillipps' *Letters Illustrative of the Progress of Science in England* (London, 1841), 116 ff.

[2] *Dictionary of National Biography*, xiii, 1299.

[3] C. F. Richardson, *English Preachers and Preaching* (New York, 1928), 177.

independently arrived at this same conclusion.[1] She points out that of the ten men who constituted the " invisible college," in 1645, only one, Scarbrough, was clearly non-Puritan. About two of the others there is some uncertainty, though Merret had a Puritan training. The others were all definitely Puritan. Moreover, among the original list of members of the Society of 1663, forty-two of the sixty-eight concerning whom the information is available were clearly Puritan. Considering that the Puritans constituted a relatively small minority in the English population, the fact that they constituted sixty-two per cent. of the initial membership of the Society becomes even more striking. Dean Stimson concludes: " that experimental science spread as rapidly as it did in seventeenth-century England seems to me to be in part at least because the moderate Puritans encouraged it."

Nor was this relationship only evidenced among the members of the Royal Society. The emphasis of the Puritans upon utilitarianism and empiricism was likewise manifested in the type of education which they introduced and fostered. The " formal grammar grind " of the schools was criticized by them as much as the formalism of the Church.

Prominent among the Puritans who so consistently sought to introduce the new realistic, utilitarian, and empirical education into England was Samuel Hartlib. He formed the connecting link between the various Protestant educators in England and in Europe who were earnestly seeking to spread the academic study of science. It was to Hartlib that Milton addressed his tractate on education, and Sir William Petty dedicated his " Advice . . . for the Advancement of some particular Parts of Learning," namely, science, technology, and handicraft. Moreover, it was Hartlib who was instrumental in broadcasting the educational ideas of Comenius and in bringing him to England.

The Bohemian Reformist, John Amos Comenius, was one

[1] Dorothy Stimson, " Puritanism and the New Philosophy in Seventeenth-century England," *Bulletin of the Institute of the History of Medicine*, iii (1935), 321–34.

of the most influential educators of this period. Basic to the system of education which he promulgated were the norms of utilitarianism and empiricism: values which could only lead to an emphasis upon the study of science and technology, of *Realia*.[1] In his most influential work, *Didactica Magna*, he summarizes his views thus:[2]

" The task of the pupil will be made easier, if the master, when he teaches him everything, show him at the same time its practical application in everyday life. This rule must be carefully observed in teaching languages, dialectic, arithmetic, geometry, physics, etc.

" . . . the truth and certainty of science depend more on the witness of the senses than on anything else. For things impress themselves directly on the senses, but on the understanding only mediately and through the senses. . . . Science, then, increases in certainty in proportion as it depends on sensuous perception."

Comenius found welcome among Protestant educators in England who subscribed to the same values; individuals such as Hartlib, John Dury, Wilkins, and Haak.[3] At the request of Hartlib, he came to England for the express purpose of making Bacon's " Solomon's House " a reality. As Comenius himself remarked: " nothing seemed more certain than that the scheme of the great Verulam, of opening in some part of the world a universal college, whose one object should be the advancement of the sciences, would be carried into effect."[4] But this aim was frustrated by the social disorder attendant upon the rebellion in Ireland. However, the Puritan design of advancing science was not entirely without fruit. Cromwell founded the only new English university instituted between the Middle Ages and the nineteenth

[1] Wilhelm Dilthey, " Pädagogik: Geschichte und Grundlinien des Systems," *Gesammelte Schriften* (Leipzig & Berlin, 1934), 163 ff.

[2] J. A. Comenius, *The Great Didactic*, translated by M. W. Keatinge (London, 1896), 292, 337; see also 195, 302, 329, 341.

[3] Robert F. Young, *Comenius in England* (Oxford, 1932), 5–9.

[4] *Opera Didactica Omnia* (Amsterdam, 1657), Book II, preface.

century, Durham University, "for all the sciences."[1] And in Cambridge, during the height of the Puritan influence there, the study of science was considerably augmented.[2]

In the same vein, the Puritan Hezekiah Woodward, a friend of Hartlib, emphasized realism (things, not words) and the teaching of science.[3] In order to initiate the study of the new science on a much more widespread scale than had hitherto obtained, the Puritans instituted a number of Dissenting Academies. These were schools of university standing opened in various parts of the kingdom. One of the earliest of these was Morton's Academy wherein there was pronounced stress laid upon scientific studies. Charles Morton later went to New England, where he was chosen vice-president of Harvard College, in which "he introduced the systems of science that he used in England."[4] At the influential Northampton Academy, another of the Puritan educational centres, mechanics, hydrostatics, physics, anatomy, and astronomy had an important place in the time-table. These studies were pursued largely with the aid of actual experiments and observations.

But the marked emphasis placed by the Puritans upon science and technology may perhaps best be appreciated by a comparison between the Puritan academies and the universities. The latter, even after they had introduced scientific subjects, continued to give an essentially classical education; the truly "cultural" studies were those which, if not entirely useless, were at least definitely non-utilitarian in purpose. The academies, in contrast, held that a truly liberal education was one which was "in touch with life" and which should therefore include as many utilitarian subjects as possible. As Dr. Parker puts it:[5]

[1] F. H. Hayward, *The Unknown Cromwell* (London, 1934), 206–30, 315.
[2] James B. Mullinger, *Cambridge Characteristics in the Seventeenth Century* (London, 1867), 180–81 *et passim*.
[3] Irene Parker, *Dissenting Academies in England* (Cambridge, 1914), 24.
[4] *Ibid.*, 62.
[5] *Ibid.*, 133–4.

" . . . the difference between the two educational systems is seen not so much in the introduction into the academies of " modern " subjects and methods as in the fact that among the Nonconformists there was a totally different system at work from that found in the universities. The spirit animating the Dissenters was that which had moved Ramus and Comenius in France and Germany and which in England had actuated Bacon and later Hartlib and his circle."

This comparison of the Puritan academies in England and Protestant educational developments on the Continent is well warranted. The Protestant academies in France devoted much more attention to scientific and utilitarian subjects than did the Catholic institutions.[1] When the Catholics took over many of the Protestant academies, the study of science was considerably diminished.[2] Moreover, as we shall see, even in the predominantly Catholic France, much of the scientific work was being done by Protestants. Protestant exiles from France included a large number of important scientists and inventors.[3]

Of course, the mere fact that an individual is *nominally* a Catholic or a Protestant has no bearing upon his attitudes toward science. It is only in so far as he adopts literally and completely the tenets and implications of the respective teachings that his religious affiliation becomes significant. For example, it was only when Pascal became thoroughly converted to the teachings of Jansenius that he perceived the " vanity of science." For Jansenius characteristically maintained that above all we must beware of that vain love of science, which though seemingly innocent, is actually a snare " leading men away from the contemplation of eternal truths to rest in the satisfaction of the finite intelligence."[4] Once Pascal was converted to such beliefs, he resolved " to make

[1] P. D. Bourchenin, *Étude sur les académies protestantes en France au XVI^e^ et au XVII^e^ siècle* (Paris, 1882), 445 *ff.*

[2] M. Nicholas, " Les académies protestantes de Montauban et de Nîmes," *Bulletin de la société de l'histoire du protestantisme français*, iv (1858), 35–48.

[3] D. C. A. Agnew, *Protestant Exiles from France* (Edinburgh, 1866), 210 *ff.*

[4] Emile Boutroux, *Pascal*, trans. by E. M. Creak (Manchester, 1902), 16.

an end of all those scientific researches to which he had hitherto applied himself."[1] Thus, it is the firm acceptance of the values basic to the two creeds which accounts for the difference in the respective scientific contributions of Catholics and Protestants.

The same association of Protestantism and science was marked in the New World. The correspondents and members of the Royal Society who lived in New England were " all trained in Calvinistic thinking."[2] " The founders of Harvard sprang from this Calvinistic culture, not from the literary era of the Renaissance or from the scientific movement of the seventeenth century, and their minds were more easily led into the latter than the former channel of thought."[3] This predilection of the Puritans for science is also noted by Professor Morison, who states: " the Puritan clergy, instead of opposing the acceptance of the Copernican theory, were the chief patrons and promoters of the new astronomy, and of other scientific discoveries, in New England."[4] It is significant that the younger John Winthrop, of Massachusetts, later a member of the Royal Society, came to London in 1641 and probably spent some time with Hartlib, Dury, and Comenius in London. Apparently, he suggested to Comenius that he come to New England and found a scientific college there.[5] Some years later, the Puritan Increase Mather, (President of Harvard College from 1684–1701) did found a " Philosophical Society " at Boston.[6]

The scientific content of Harvard's educational programme

[1] Emile Boutroux, *Pascal*, trans. by E. M. Creak (Manchester, 1902), 17; *cf.* Jacques Chevalier, *Pascal* (New York, 1930), 143; Pascal's *Pensées*, trans. by O. W. Wight, (Boston, 1884), 224, No. xxvii. " *Vanity of the Sciences.* The science of external things will not console me for ignorance of ethics in times of affliction; but the science of morals will always console me for ignorance of external sciences."

[2] Stimson, *op. cit.*, 332.

[3] Porter G. Perrin, " Possible Sources of *Technologia* at Early Harvard," *New England Quarterly*, vii (1934), 724.

[4] Samuel E. Morison, " Astronomy at Colonial Harvard," *New England Quarterly*, vii (1934), 3–24; also Clifford K. Shipton, " A Plea for Puritanism," *The American Historical Review*, xl (1935), 463–4.

[5] R. F. Young, *Comenius in England*, 7–8.

[6] *Ibid.*, 95.

derived greatly from the Protestant Peter Ramus.[1] Ramus had formulated an educational curriculum which in contrast to that of the Catholic universities lay great stress on the study of the sciences.[2] His ideas were welcomed in the Protestant universities on the Continent, at Cambridge (which had a greater Puritan and scientific element than Oxford),[3] and later at Harvard, but were firmly denounced in the various Catholic institutions.[4] The Reformation spirit of utilitarianism and "realism" probably accounts largely for this favourable reception of Ramus' views.

Dr. Parker notes that the Puritan academies in England "may be compared with the schools of the Pietists in Germany, which under Francke and his followers prepared the way for the *Realschulen*, for there can be no doubt that just as the Pietists carried on the work of Comenius in Germany, so the Dissenters put into practice the theories of Comenius' English followers, Hartlib, Milton, and Petty."[5] The significance of this comparison is profound, for as has been frequently observed, the values and principles of Puritanism and Pietism are almost identical. Cotton Mather had recognized the close resemblance of these two Protestant movements, saying that "ye American puritanism is so much of a piece with ye Frederician pietism" that they may be considered as virtually identical.[6] Pietism, except for its

[1] Perrin, *op. cit.*, 723–4.

[2] Theobald Ziegler, *Geschichte der Pädagogik* (München, 1895), i, 108. Ziegler indicates that while the contemporary French Catholic institutions only devoted one-sixth of the curriculum to science, Ramus dedicated fully one-half to scientific studies.

[3] David Masson properly calls Cambridge the alma mater of the Puritans. In listing twenty leading Puritan clergymen in New England, Masson found that seventeen of them were alumni of Cambridge, while only three came from Oxford. See his *Life of Milton* (London, 1875), ii, 563; cited by Stimson, *op. cit.*, 332. See also *A History of the University of Oxford*, by Charles E. Mallet (London, 1924), ii, 147.

[4] Heinrich Schreiber, *Geschichte der Albert-Ludwigs-Universität zu Freiburg* (Freiburg, 1857–68), ii, 135. For example, at the Jesuit university of Freiburg, Ramus could only be referred to if he were refuted, and "no copies of his books are to be found in the hands of a student."

[5] Parker, *op. cit.*, 135.

[6] Kuno Francke, "Cotton Mather and August Hermann Francke," *Harvard Studies and Notes*, v (1896), 63. See also the cogent discussion of this point by Max Weber, *Protestant Ethic*, 132–5.

greater "enthusiasm," might almost be termed the Continental counterpart of Puritanism. Hence, if our hypothesis of the association between Puritanism and interest in science and technology is warranted, one would expect to find the same correlation among the Pietists. And such was markedly the case.

The Pietists in Germany and elsewhere entered into a close alliance with the "new education": the study of science and technology, of *Realia*.[1] The two movements had in common the realistic and practical point of view, combined with an intense aversion to the speculation of Aristotelian philosophers. Fundamental to the educational views of the Pietists were the same deep-rooted utilitarian and empirical values which actuated the Puritans.[2] It was on the basis of these values that the Pietist leader, August Hermann Francke, Comenius, and their followers emphasized the new science.

Francke repeatedly notes the desirability of acquainting students with practical scientific knowledge.[3] Both Francke and his colleague, Christian Thomasius, set themselves in opposition to the strong educational movement developed by Christian Weise, which advocated primarily training in oratory and classics, and sought rather "to introduce the neglected modern disciplines, which served their purposes more adequately; such studies as biology, physics, astronomy, and the like."[4]

[1] Friedrich Paulsen, *German Education: Past and Present*, trans. by T. Lorenz (London, 1908), 104 *ff.*

[2] Alfred Heubaum, *Geschichte des deutschen Bildungswesen seit der Mitte des siebzehnten Jahrhunderts* (Berlin, 1905), i, 90. "Ziel der Erziehung [among Pietists] ist praktische Venwendbarkeit des Zöglings im Gemeinwohl." "Der starke Einfluss des utilitaristischen Moments . . . vermindert die Gefahr der Uebertreibung des religiösen Moments und sichert der Bewegung für die nächste Zukunft ihre Bedeutung."

[3] During walks in the field, says Francke, the instructor should "nützliche und erbauliche Geschichten erzählen oder etwas aus der Physik von den Geschöpfen und Werken Gottes vorsagen." ". . . in Naturalienkabinet diente dazu, die Zöglinge in ihren Freistunden durch den Anstaltarzt mit naturwissenschaftlichen Erscheinungen, mit Mineralien, Bergarten, hie und da mit Experimenten bekannt zu machen." Quoted by Heubaum, *op. cit.*, i, 89, 94.

[4] Heubaum, *op. cit.*, i, 136.

Wherever Pietism spread its influence upon the educational system there followed the large-scale introduction of scientific and technical subjects.[1] Thus, Francke and Thomasius built the foundations of the University of Halle, which was the first German university to introduce a thorough training in the sciences.[2] The leading professors, such as Friedrich Hoffman, Ernst Stahl (professor of chemistry and famous for his influential phlogiston theory), Samuel Stryk, and, of course, Francke, all stood in the closest relations with the Pietistic movement. All of them characteristically sought to develop the teaching of science and to ally science with practical applications.

Not only Halle, but other Pietistic universities manifested the same emphases. Königsberg, having come under the Pietistic influence of the University of Halle through the activities of Francke's disciple, Gehr, early adopted the natural and physical sciences in the modern sense of the seventeenth century.[3] The University of Göttingen, an "offshoot" of Halle, was famous essentially for the great progress which it effected in the cultivation of the sciences.[4] The Calvinistic university of Heidelberg was likewise prominent for instituting such a large measure of scientific study.[5] Finally, the University of Altdorf, which was at that time the most conspicuous for its interest in science, was a Protestant University subject to Pietistic influence.[6] Heubaum summarizes these developments by asserting that the essential progress in the teaching of science and technology occurred in Protestant, and more precisely, in Pietistic universities.[7]

[1] Heubaum, *op. cit.*, i, 176*ff*.

[2] Koppel S. Pinson, *Pietism as a Factor in the Rise of German Nationalism* (New York, 1934), 18; Heubaum, *op. cit.*, i, 118. "Halle war die erste deutsche Universität von ganz eigenartigem wissenschaftlichen und nationalen Gepräge . . ."

[3] Heubaum, *op. cit.*, i, 153.

[4] Paulsen, *op. cit.*, 120–1.

[5] Heubaum, *op. cit.*, i, 60.

[6] S. Günther, "Die mathematischen und Naturwissenschaften an der nürnbergischen Universität Altdorf," *Mitteilungen des Vereins für Geschichte der Stadt Nürnberg*, Heft. iii, 9.

[7] Heubaum, *op. cit.*, i, 241; see also Paulsen, *op. cit.*, 122; J. D. Michaelis, *Raisonnement über die protestantischen Universitäten in Deutschland* (Frankfurt, 1768), i, section 36.

This association of Pietism and science, which we have been led to anticipate from our hypothesis, did not confine itself to the universities. The same Pietist predilection for science and technology was evidenced in secondary school education. The *Pädagogium* of Halle introduced the subjects of mathematics and natural science; stress being laid, in all cases, on the use of object lessons and on practical applications.[1] Johann George Lieb, Johann Bernhard von Rohr, and Johann Peter Ludewig (Chancellor of Halle University), all of whom had come under the direct influence of Francke and Pietism, advocated schools of manufacture, physics, mathematics, and economics, in order to study how "manufacture might be ever more and more improved and excelled."[2] They hoped that the outcome of these suggestions might be a so-called *Collegium physicum-mechanicum* and *Werkschulen*.

It is a significant fact, and one which lends additional weight to our hypothesis, that the *ökonomisch-mathematische Realschule* was completely a Pietist product. This school, which was centred about the study of mathematics, the natural sciences, and economics, and which was avowedly utilitarian and realistic in temper, was planned by Francke.[3] Moreover, it was a Pietist and a former student of Francke, Johann Julius Hecker, who first actually organized a *Realschule*.[4] Semler, Silberschlag, and Hähn, the directors and co-organizers of this first school, were all Pietists and former students of Francke.[5]

[1] Paulsen, *op. cit.*, 127.

[2] Heubaum, *op. cit.*, i, 184.

[3] Alfred Heubaum, "Christoph Semlers Realschule und seine Beziehung zu A. H. Francke," *Neue Jahrbücher für Philologie und Pädagogik*, ii (1893), 65–77; see also Ziegler, *Geschichte der Pädagogik*, i, 197, who observes: ". . . einen inneren Zusammenhang zwischen der auf das Praktische gerichteten Frömmigkeit der Pietisten fehlte es ja auch nicht, nur eine ganz einseitig religiöse und theologische Auffassung des Pietismus kann das verkennen: im Geist der praktischen Nützlichkeit und Gemeinnützigkeit ist dieser dem Rationalismus vorangegangen, und mit ihm eins gewesen, und aus diesem Geist heraus ist zu Franckes Zeiten in Halle die Realschule entstanden."

[4] Paulsen, *op. cit.*, 133.

[5] Upon the basis of this and other facts, Ziegler proceeds to trace a close "Kausalzusammenhang" between Pietism and the study of science. See his *Geschichte*, i, 196, *ff.*

All available evidence points in the same direction. Protestants, without exception, form a progressively larger proportion of the student body in those schools which emphasize scientific and technologic training,[1] while Catholics concentrate their interests on classical and theological training. For example, in Prussia the following distribution was found.[2]

ATTENDANCE AT SECONDARY SCHOOLS DIFFERENTIATED BY RELIGIOUS AFFILIATIONS OF THE STUDENTS

PRUSSIA, 1875–6

Religious Affiliation.	Pro-gymnasium.	Gymnasium.	Real-schule.	Ober-realsch.	Höheren Burger.	Total.	General Population.
Protestants .	49·1	69·7	79·8	75·8	80·7	73·1	64·9
Catholics .	39·1	20·2	11·4	6·7	14·2	17·3	33·6
Jews . .	11·2	10·1	8·8	17·5	5·1	9·6	1·3

This greater propensity of the Protestants for the scientific and technical studies accords with the implications of our hypothesis. That this distribution is typical may be gathered from the fact that other investigators have noted the same tendency in other instances.[3] Furthermore, these distribu-

[1] The characteristic feature of the *gymnasien* is the classical basis of their curricula. Demarcated from these schools are the *Realschulen*, where the sciences predominate and where modern languages are substituted for the classical tongues. The *Real-gymnasium* is a compromise between these two types, having less classical instruction than the *gymnasium* with more attention paid to science and mathematics. The *Ober-realschulen* and *höheren Bürgerschulen* are both *Realschulen;* the first with a nine-year course, the second with a six-year course. *Cf.* Paulsen, *German Education*, 46 *et passim.*

[2] Alwin Petersilie, "Zur Statistik der höheren Lehranstalten in Preussen," *Zeitschrift des königlich Preussischen Statistichen Bureaus*, xvii (1877), 109.

[3] Edouard Borel, *Religion und Beruf* (Basel, 1930), 93*ff.*, who remarks the unusually high proportion of Protestants in the technical professions in Basel; Julius Wolf, "Die deutschen Katholiken in Staat und Wirtschaft," *Zeitschrift für Sozialwissenschaft*, iv (1913), 199, notes that "die Protestanten ihren 'naturgemassen' Anteil überschreiten gilt für die wissenschaftliche und sonstige intellektuelle Betätigung (mit Ausnahme des geistlichen Berufs) . . ." In 1860, Ad. Frantz had already noted the same fact. See his "Bedeutung der Religionunterschiede für das physische Leben der Bevölkerungen," *Jahrbücher für Nationalökonomie und Statistik*, xi (1868), 51. *Cf.* also similar results for Berlin in *Statistisches Jahrbuch der Stadt Berlin*, xxii (1897), 468–72. Buckle, *op. cit.*, 482, notes that "Calvinism is favourable to science," *Cf.* also Weber, *Protestant Ethic*, 38, 189; and Troeltsch, *Social Teachings* . . . , ii, 894.

tions do not represent a spurious correlation resulting from differences in rural-urban distribution of the two religions, as may be seen from the pertinent data for the Swiss canton, Basel-Stadt. As is well known, the urban population tends to contribute more to the fields of science and technology than the rural. Yet for 1910 and following—the period to which Edouard Borel's study, with results similar to those just presented for Prussia, refers—Protestants constituted 63·4 per cent. of the total population of the canton, but only 57·1 per cent. of the population of Basel (the city proper) and 84·7 per cent. of the rural population.[1]

Martin Offenbacher's careful study includes an analysis of the association between religious affiliation and the allocation of educational interests in Baden, Bavaria, Württemberg, Prussia, Alsace-Lorraine, and Hungary. The statistical results in these various places are of the same nature: Protestants, proportionately to their representation in the population at large, have a much higher attendance at the various secondary schools, with the difference becoming especially marked in the schools primarily devoted to the sciences and technology. In Baden,[2] for example, taking an average of the figures for the years 1885-95:

	Protestants. per cent.	Catholics. per cent.	Jews. per cent.
Gymnasien	43	46	9·5
Realgymnasien	69	31	9
Oberrealschulen	52	41	7
Realschulen	49	40	11
Höheren Bürgerschulen	51	37	12
Average for the five types of schools	48	42	10
Distribution in the general population, 1895	37	61·5	1·5

However, it must be noted that although the *Realschulen* curricula are primarily characterized by their stress on the

[1] See "Die Bevölkerung des Kantons Basel-Stadt," *Mitteilungen des Statistischen Amtes des Kantons Basel-Stadt* (1932), 48–49; and the same publication for the years 1910 and 1921.

[2] Martin Offenbacher, *Konfession und soziale Schichtung* (Tübingen, 1900), 16.

sciences and mathematics as contrasted with the relatively little attention paid these studies in the *gymnasien*, yet the latter type of school also prepares for scientific and scholarly careers. But, in general, the attendance of Protestants and Catholics at the *gymnasien* represents different interests. The relatively high incidence of Catholics at the *gymnasien* is due to the fact that these schools prepare for theology as well, while the Protestants generally use the *gymnasien* as a preparation for the other learned professions. Thus, in the three academic years, 1891-4, 226, or over 42 per cent. of the 533 Catholic graduates of the Baden *gymnasien* subsequently studied theology, while of the 375 Protestant graduates, only 53 (14 per cent.) turned to theology, while 86 per cent. went into the other learned professions.[1]

Similarly, the Catholic apologist, Hans Rost, though he wishes to establish the thesis that " the Catholic Church has been at all times a warm friend of science," is forced to admit, on the basis of his data, that the Catholics avoid the *Realschulen*, that they show " eine gewisse Gleichgultigkeit und Abneigung gegen diese Anstalten." The reason for this, he goes on to say, is " dass die Oberrealschule und das Realgymnasium nicht zum Stadium der Theologie berechtigen: denn diese ist häufig die Triebfeder bei den Katholiken zum höheren Studium überhaupt." [2]

Thus, statistical data point to a marked tendency for Protestants, as contrasted with Catholics, to pursue scientific and technical studies. This can also be seen in the statistics for Württemberg, where an average of the years 1872-9 and 1883-98 gives the following figures: [3]

[1] H. Gemss, *Statistik der Gymnasialabiturienten in deutschen Reich* (Berlin, 1895), 14–20.

[2] Hans Rost, *Die wirtschaftliche und kulturelle Lage der deutschen Katholiken* (Koln, 1911), 167 *ff.*

[3] Offenbacher, *op. cit.*, 18. These data are corroborated by the study of Ludwig Cron pertaining to Germany for the years 1869–93; *Glaubenbekenntnis und höheres Studium* (Heidelberg, 1900). Ernst Engel also found that in Prussia, Posen, Brandenburg, Pomerania, Saxony, Westphalia, and the Rhine Provinces, there is a higher incidence of Evangelical students in these schools which provide a maximum of natural science and technica subjects. See his " Beiträge zur Geschichte und Statistik des Unterrichts," *Zeitschrift de königlich Preussischen statistischen Bureaus*, ix (1869), 99–116, 153–212.

	Protestants. per cent.	Catholics. per cent.	Jews. per cent.
Gymnasien	68·2	28·2	3·4
Lateinschulen	73·2	22·3	3·9
Realschulen	79·7	14·8	4·2
Total population, 1880 . . .	69·1	30·0	·7

Nor do the Protestants evidence these foci of interest only in education. Various studies have found an unduly large representation of Protestants among outstanding scientists.[1] If the foregoing data simply provide slight probabilities that the connexion we have traced does in fact obtain, Candolle's well known *Histoire des sciences et des savants* increases these probabilities considerably. Candolle finds that although in Europe, excluding France, there were 107 million Catholics and 68 million Protestants, yet on the list of scientists named foreign associates by the Academy of Paris from 1666–1883, there were only eighteen Catholics as against eighty Protestants![2] But as Candolle himself suggests, this comparison is not conclusive since it omits French scientists who may have been Catholic. To correct this error, he takes the list of foreign members of the Royal Society of London at two periods when there were more French scientists included than at any other time: 1829 and 1869. In the former year, the total number of Protestant and Catholic scientists (who are foreign members of the Society) is about equal, while in 1869, the number of Protestants actually exceeds that of Catholics. But, outside the kingdom of Great Britain and Ireland, there were in Europe 139½ million Catholics and only 44 million Protestants![3] In other words, though in the

[1] For example, Havelock Ellis' *Study of British Genius*, 66 *ff.* finds that Protestant Scotland produced twenty-one of the outstanding scientists on his list as against one for Catholic Ireland. Alfred Odin finds that among the littérateurs on his list, the predominant emphasis of Protestants is on scientific and technical subjects, rather than on literature, properly so-called. See his *Genèse des grands hommes* (Paris, 1895), i, 477 *ff*, ii, Tables xx–xxi.

[2] Alphonse de Candolle, *Histoire des sciences et des savants* (Geneva-Basel, 1885), 329.

[3] *Ibid.*, 330. *Cf.* Făcăoaru, *Soziale Auslese* (Klausberg, 1933), 138–9. "Die Konfession hat einem grossen Einfluss auf die Entwicklung der Wissenschaft gehabt. Die Protestanten wiesen überall eine grössere Zahl hervorragender Männer auf."

general population there were more than three times as many Catholics as Protestants, there were actually more Protestant than Catholic scientists.

However, there are yet more significant data than these which are based on different populations, where influence of climate, political regime, and other non-religious factors may be suspected to prevail over the actual influence of religion. A comparison of closely allied populations serves largely to eliminate these "extraneous" factors, but the results are the same. Thus, on the list of foreign associates of the Academy of Paris, there is not a single Irish or English Catholic, although their proportion in the population of the three kingdoms exceeded a fifth. Likewise, Catholic Austria is not at all represented, while in general Catholic Germany is similarly lacking in the production of scientists of note relative to Protestant Germany. Finally, in Switzerland, where the two religions are largely differentiated by cantons, or mixed in some of them, and where the Protestants are to the Catholics as three to two there have been fourteen foreign Associates, of whom not one was Catholic! The same differentiation exists for the Swiss and for the English and Irish of the two religions in the lists of the Royal Society of London and the Royal Academy of Berlin.[1]

With the presentation of these data we close the empirical testing of our hypothesis. In every instance, the association of Protestantism with scientific and technologic interests and achievements is pronounced, even when extra-religious influences are largely eliminated. The association is largely understandable in terms of the norms embodied in both systems. The positive estimation by Protestants of a hardly disguised utilitarianism, of intra-mundane interests, of a thorough-going empiricism, of the right and even duty of *libre examen*, and of the explicit individual questioning of authority were congenial to the very same values found in

[1] Alphonse de Candolle, *Histoire des sciences et des savants* (Geneva-Basel, 1885), 330*ff.*

modern science. And perhaps above all is the significance of the active ascetic drive which necessitated the study of Nature that it might be controlled. Hence, these two fields were well integrated and, in essentials, mutually supporting, not only in seventeenth-century England but in other times and places.

LOMBROSO AND HIS PLACE IN MODERN CRIMINOLOGY

By HERMANN MANNHEIM

IT would not be surprising if the centenary of Lombroso's birthday[1] were to provide an opportunity for reviewing his work. It is, however, the strange rôle that Lombroso plays within the criminological science of to-day which, much more than that external event, renders necessary an attempt to answer the question: Are we at present entitled to speak of a Lombroso-Renaissance—are we really going "back to Lombroso"? It is, therefore, intended in the present article—instead of merely augmenting the huge number of descriptions of Lombroso's theory already in existence—to examine the actual significance of his doctrines to modern Criminology. Let us for a moment recall to our minds the main stages of his scientific career. After an enthusiastic reception by admirers all over the world[2] his opponents very soon gained strength. It was, on the one hand, the supporters of Indeterminism[3] in philosophy and jurisprudence that, feeling menaced by the aspects opened by the Positive School, joined the inveterate adversaries of Criminal Anthropology, the crimino-sociologist leaders, Franz von Liszt and others. But, it was, above all, the anthropologists themselves that abhorred Lombroso's unscientific and uncritical methods and his perpetually changing formulations. In 1893 Adolf

[1] There is apparently some divergence of opinion as to this date. Lombroso's daughter, Gina Lombroso-Ferrero, and the Enciclopedia Italiana state November 6, 1835, Brockhaus' *Konversations Lexikon* November 18, 1836, Aschaffenburg (*Handwörterbuch der Kriminologie*—abbr. *Hdk.*—1, p. 833) even 1832. The first date may be right.

[2] See, e.g. Havelock Ellis, *The Criminal* (1. ed., 1890); Hans Kurella, *Naturgeschichte des Verbrechers* (1893); Eugen Bleuler, *Der geborene Verbrecher* (1896).

[3] See, e.g. Enrico Ferri, *Criminal Sociology* (American edition, 1917), pp. 288 *et seq.* and the indications made by Ch. Goring, *The English Convict*, p. 13, Hans Gross, *Handbuch für Untersuchungsrichter* (7 ed. 1922), vol. i, p. 153. Lombroso himself replied to the attacks of Indeterminism in the preface to the later editions of his *L'Uomo Delinquente* (see, e.g. the German edition: *Der Verbrecher in anthropologischer, ärztlicher, und juristischer Beziehung*, translated by O. Fraenkel, 1887, vol. i, pp. xxvi *et seq.*

Baer, the Berlin prison surgeon, published his profound and unbiased work *Der Verbrecher in anthropologischer Beziehung*, and simultaneously Naecke, in several essays, tried to disprove Lombroso's statements.[1] In 1913 these works were followed by Charles Goring's *English Convict*, the most comprehensive and painstaking of all existing refutations of the conception of the born criminal. Relying on these researches the criminologists of the early twentieth century became accustomed to disdain Lombroso's theories. "If we ask how the Lombroso question stands to-day," writes Hans Gross,[2] "we may say that it is finished," or "the dream of the 'born criminal', 'the natural delinquent' as a special human type has been dreamed to a finish. Nothing remains of it except the sterile fact that a large number of criminals are in some way or another abnormal, just as are many other people who never come into conflict with the law at all."[3] On account of such statements, it may perhaps seem rather superfluous to waste even a single line upon Lombroso. But are they completely correct, and how far are they in harmony with the fact that even in modern Criminology scarcely any other author is the subject of so many heated discussions and disputes as Lombroso? If his ideas are really dead, why not leave him in peace?

In the history of science it is always essential to distinguish between the gist of a theory, i.e. its enduring principles, and its accidental and transient manifestations. Who, to-day, has the right to reproach the inventor of the steam-engine for not having produced at once a perfect model in the light of modern technology? It is Lombroso's lasting merit—and nobody should acknowledge it with deeper gratitude than the jurist—that he has saved criminal science from the shackles of

[1] See, moreover, Manouvrier, *Les Aptitudes et les Actes* (Paris, 1893); Jules Dallemagne, *Les stigmates anatomiques de la criminalité* (1896), and *Les stigmates biologiques et sociologiques de la criminalité* (1896).

[2] *Op. cit.*, p. 158.

[3] Johannes Lange, *Verbrechen als Schicksal*, quoted from the English translation by Charlotte Haldane, *Crime as Destiny* (London, George Allen & Unwin, Ltd.), 1931, p. 30.

merely academic abstractions and that he fertilized it with the rich treasure of the natural sciences. Previous to Lombroso's appearance criminal science, especially on the Continent, was entirely dominated by the Classical School of Beccaria, Feuerbach, etc. It contented itself with endless theoretical arguments and tried by means of sterile conceptions to fix the penalty which was supposed to " fit the crime." It was the criminal action alone that was examined as if it stood completely isolated from the other actions of its author. Consideration of the personality of the individual law-breaker was nobody's concern.[1] The criminal, it was thought, differed in no way from the average law-abiding citizen; he might just as easily have chosen an honest career if only he had wished to do so.[2] Typical in this respect was the attitude of the former Italian (Fr. Carrara) and German (Hegelian School) criminal science. Feuerbach, it is true, although himself a chief representative of the Classical School, had already in 1828–9 produced his *Aktenmässige Darstellung merkwürdiger Criminalrechtsfälle* (Remarkable criminal cases, compiled from records of the courts), which was succeeded by several similar works. But these publications, meritorious as they were, suffered from the absence of direct observation of the criminal and were unable of themselves to change the fundamental attitude towards the treatment of crime. The inevitable results of these methods duly came to light: the figures of recidivism all over Europe in the second half of the nineteenth century were sufficiently alarming[3] and proved to be the best supporters of Lombroso. " The principle that the criminal himself, and not an abstract idea of crime, should be punished was not invented by the Italian School," wrote Franz von Liszt in his programmatical lecture on " The tasks of Criminal Policy " (1889),[4] " but the Italians recalled it to

[1] See E. Ferri, *Criminal Sociology* (American edition), pp. 4 and 43.

[2] See G. Daniel, *Zeitschrift für die gesamte Strafrechtswissenschaft*, vol. I, p. 478.

[3] See, e.g., Lombroso's own figures: *L'Uomo delinquente* (German edition), i, pp. 334 *et seq.*; Sir Evelyn Ruggles Brise, *The English Prison System* (1921), pp. 5, 221, etc.; v. Liszt, *Strafrechtliche Aufsätze und Vorträge*, vol. ii, pp. 238, 338.

[4] *Op. cit.*, vol. i, p. 307.

our minds at a time when Kant and Hegel, Fichte and Herbart, Schopenhauer and Ed. von Hartmann made us forget this fundamental aim of all criminal science. They stirred us up from our metaphysical sleep and from our paralysing theoretical jurisprudence; somewhat rudely and with unnecessary noise, but with lasting success." It is true that Lombroso had numerous forerunners, not only among the jurists interested in the psychological investigation of crime, but also in his original field of science. Apart from the self-evident fact that Lombroso is almost unthinkable without Darwin, the fact remains that the special branch of Criminal Anthropology, too, was not created by him out of nothing. In this connexion it may suffice to mention the names of Broca and Dr. G. Wilson.[1] "The science of Criminal Anthropology," admits Ferri,[2] "only begins with the observations of English gaol surgeons and other learned men." Nobody, however, was engaged in Crimino-Anthropological research with such intensive energy and enthusiasm; nobody knew, as Lombroso did, how to popularize its facts and results. If Lombroso, to take only one instance, in a review of an essay on the "*vagabond-né*," writes :[3] "Chaque jour on découvre quelques espèces nouvelles, quelque nouveau type de criminels"; such words express a delight in the progress of his science, which was certainly very naïve, but nevertheless apt to enrapture the uncritical public—a delight almost comparable to Ulrich von Hutten's "O wonderful century, the mind's awake—it is a joy to live!" Did Lombroso's enthusiasm, however, really succeed in discovering the *special criminal types* that he sought, and are these types capable of definition according to *anthropological* characteristics? The almost unanimous opinion of to-day, as is well known, answers the second question in the negative. It is especially Lombroso's identification of the born criminal type with the

[1] See the more detailed account given by Havelock Ellis, *The Criminal*, p. 37; K. Birnbaum, *Kriminalpsychopathologie* (1921), p. 142.

[2] *Criminal Sociology* (English edition, 1895), p. 3; see p. 42 of the American edition.

[3] *Nouvelles Recherches de Psychiatrie et d'Anthropologie Criminelle* (Paris 1892), p. 65.

notions of atavism and degeneration; the exceeding importance which he attributed to epilepsy; his belief in an anthropological criminal type of even international uniformity[1] that are commonly rejected. The main features of his theory of the anthropological stigmata of the born criminal have been disproved by Baer and Goring, who examined the problem independently.[2] Many of the statements made by Lombroso in this respect are to-day at best no more than curiosities,[3] and even so far as they are partly confirmed by later researches,[4] they cannot be regarded as sufficient foundations for his sweeping conclusions.

In spite of this rejection, however, there is a considerable change of tone perceptible in the last years. It is interesting to compare, for example, Goring's criticism with that of modern authoritative writers. Goring, it is true, also endeavoured to do justice to Lombroso, emphasizing that his struggle was directed not so much against Lombroso's results as against the methods by which they were achieved.[5] But, Goring stated, Lombroso's importance lay not in his scientific work, but in his humanitarianism.[6] To a man of Goring's dispassionate objectivity, who approved of nothing but exact statistical data and measurements,[7] the fanciful anecdotical style of the Italian scholar could only be repulsive. Nothing is more characteristic in this respect than the sarcasm which he poured upon Lombroso's description of the birth of his theory: "Newton must work by other laws than Victor

[1] It should, however, not be overlooked that Lombroso recognized certain national and regional peculiarities (see, e.g., *L'Uomo delinquente*, vol. i, pp. 152 and 213 of the German translation).

[2] Baer's investigations were obviously unknown to Goring.

[3] So when he stated that the amount of nitrogen in the urine of the born criminal was 0·38 in every kilogram, as compared with 0·82 in case of the occasional criminal (*L'Uomo delinquente*, German edition ii, p. 301.

[4] E.g. the frequency of epilepsy among criminals (Baer, pp. 300 *et seq.*; Goring, pp. 226 *et seq.*).

[5] *The English Convict*, p. 19.

[6] *Op. cit.* p. 12.

[7] See, e.g. *op. cit.* p. 29. "We must build upon measurements and upon measurements alone."

Hugo."[1] As a result of the Lombroso-Goring controversy, whoever has lost his belief in the reality of the born-criminal type must at least have become convinced of the existence of absolutely different types of scientists. To-day, one is more inclined to tolerate the methods of Lombroso—in full acknowledgment of their shortcomings—as "ingenious intuition."[2] The deeper reasons for this reaction are, of course, to be found in the growing insight in the limitations of the method of "measurements and measurements alone,"[3] and, moreover, in the fact that some previous critics had attacked many alleged statements of Lombroso which the latter had, actually, either not made at all or made in another sense.

At present, more than ever are being emphasized the important reservations by means of which Lombroso, in the course of years, tried to support his theory: his restriction (from the third edition of *L'Uomo delinquente*) of the conception of the born criminal to about 35 per cent. of all criminals; his requirement of at least five stigmata for this type; his readiness to acknowledge the importance of mental, social, economic, cosmic, etc., factors in addition to the anthropological ones;[4] finally, the practicability of many of his suggestions for penological reforms. And already at an earlier date Ferri,[5] rightly or wrongly, had emphasized that the contrast between the born and the occasional criminal, according to Lombroso's own statements, was to be regarded only as "a difference of degree and modality."

In his native country, the influence of Lombroso's ideas is

[1] *Op cit.*, p. 13, fn. 2. See, on the other hand, the remarks by Professor Leone Lattes (Pavia) in *La Giustizia Penale*, vol. xl, *Parte Prima* (1934), p. 1: "Onde si iniziò la grandiosa architettura scientifica che rese celebre ovunque il suo nome."

[2] See, e.g., Aschaffenburg, *Handwörterbuch der Kriminologie*, i, p. 834.

[3] Goring himself is being subjected to the reproach that his methods, according to present conceptions, are inadequate (see Sutherland, *Principles of Criminology*, 1934, pp. 47, 55 *et seq.*

[4] See Birnbaum, *op. cit.*, p. 146; O. Kinberg, *Basic Principles of Criminology* (1935), 166; Gruhle, *HdK.* i, p. 910; Goring, *op. cit.*, p. 263.

[5] *Criminal Sociology* (English edition), p. 43.

very strong even to-day.[1] Among Italian scholars, the views of the well-known psychiatrist Professor B. di Tullio (Rome) appear nowadays[2] to be very similar to those of Lombroso. "If we sum up," he writes in a short study which summarizes his investigations,[3] "the results of our examinations of pickpockets, burglars, hotel thieves, swindlers, sexual offenders, and juvenile delinquents, we venture to say that all genuine[4] criminals show constitutionally abnormal features, although in varying degrees. The genuine criminal always shows endogenous defects of development, the latter having been arrested at a past stage of mankind, and he thereby resembles primitive, even savage tribes, etc."[5]

Some of the fundamental ideas of Lombroso have gained admittance even into the new Italian Penal Code of 1931. It is especially art. 108 of this Code[6] that is worth mentioning in this connexion, because it deals with the so-called criminal by tendency as follows: "A person who, although neither a relapser nor a habitual or professional delinquent, commits a crime, not without criminal intent, against individual life or safety . . . which . . . discloses a special inclination to crime, the cause of which is the particularly wicked nature of the guilty party, shall be declared a criminal by tendency."[7]

[1] Very significant is in this respect Prof. Leone Lattes' article in *La Giustizia Penale*, vol. xl, Parte Prima, (1934), pp. 1 *et seq.* Prof. Thorsten Sellin (Philadelphia) gives an excellent survey of the position of Criminal Anthropology and especially Endocrinology in Italy ten years ago (The Annals of the American Academy of Political and Social Science, May 1926, pp. 233 *et seq.*). See, moreover, Prof. Q. Saldaña (Madrid), *Journal of Criminal Law and Criminology*, vol. xxiv, pp. 333 *et seq.*

[2] Lattes, *op. cit.*, p. 4, calls him "più recente esponente della scuola lombrosiana."

[3] *Zeitschrift für die gesamte Strafrechtswissenschaft*, vol. l (1930), pp. 493-4.

[4] What does this expression mean? (The German original says "echt.")

[5] Nevertheless, as Prof. N. Cantor points out in his interesting review (*American Journal of Sociology*, vol. xxxix, p. 713), di Tullio's conception of the criminal type is dynamic, whilst that of Lombroso was static. The same contrast is stressed by Edmund Mezger, *Die Arbeitsmethoden der kriminalbiologischen Untersuchungsstellen, Der Gerichtssaal*, vol. ciii, pp. 127 *et seq.*

[6] Di Tullio, *Monatsschrift für Kriminalpsychologie und Strafrechtsreform*, vol. xxii (1931), p. 345, regards art. 108 as a direct approbation of Lombroso's theory. On the other hand, see Lattes, *op. cit.*, p. 15.

[7] Official English translation. Foreign Office, 1931.

Such a declaration " shall entail the application of police measures " (art. 109).

In Germany, Austria, and some other Continental countries, Lombroso's school has been succeeded by the so-called Crimino-biological School, which latter has utilized the opportunities for research offered by the establishment of several Institutes for crimino-biological investigation. This School has its scientific centre in the *Kriminalbiologische Gesellschaft*, founded by Professor Adolf Lenz in Graz,[1] and, since 1927, in the *Mitteilungen*, edited by this Society. Some of its members, it is true, are inclined to lay more stress upon the differences which separate them from Lombroso than upon the points in which they are in harmony with him. The Crimino-biological School owes—if not its origin—at any rate its attractive force mainly to the famous researches of Professor Ernst Kretschmer.[2] The latter, as is well known, has established his constitutional types regardless of the needs of Criminology; neither has he limited them to abnormal persons. As a psychiatrist, he endeavours, of course, to discover connexions with corresponding psychiatric types, especially with Kraepelin's distinction between *manic-depressive* or *circular* insanity, on the one hand, and *schizophrenia* or *dementia praecox* on the other.[3] He extends, however, the main features of this contrast to two other pairs of types ; First, the *cycloid* and the *schizoid* types which consist of individuals who stand between sickness and health and exhibit the

[1] See his *Grundriss der Kriminalbiologie* (1927).

[2] *Körperbau und Charakter*, 9–10 ed., 1931. The English translation, under the title " Physique and Character," was published in 1925 (International Library of Pyschology etc.). I am well aware of the fact that the Crimino-biological School by no means utilizes Kretschmer's methods alone. Nevertheless, it is impossible to deny the preponderating influence of the latter. As to the relations between Kretschmer's work and the Crimino-biological School, see esp. E. Mezger, *Kriminalpolitik* (1934), pp. 67–139; E. Mezger in *Der Gerichtssaal*, vol. ciii, p. 127; W. Sauer, *Kriminalsoziologie*, vol. i (1933), p. 26; Adolf Lenz, *Grundriss der Kriminalbiologie* (1927); W. Sauer, *Archiv der Rechts—und Wirtschaftsphilosophie*, vol. xxiii (1929–30), p. 430; von Rohden, *Einführung in die kriminalbiologische Methodenlehre* (1933); Willemse, Constitution-Types in Delinquency (London, 1932); Reiter, *Monatsschrift für Kriminalpsychologie und Strafrechtsreform*, vol. xxii, p. 78; Aschaffenburg, *HdK.* i, pp. 834 *et seq.*

[3] See the short definitions given in the Report on Sterilization (1934), p. 77.

psychic symptoms of the distinctly abnormal circular and schizophrene types in a slighter degree.[1] Secondly: the *cyclothymes* and the *schizothymes*, i.e. the corresponding *normal* temperaments from which the pathological types are recruited.[2] Kretschmer, moreover, attempts to combine these pyschological and pathological characteristics with certain bodily features, the asthenic,[3] the athletic and the pyknic types. His final results are commonly known. Among the schizophrenes, schizoids, and schizothymes, he states, the asthenic and athletic types prevail, as does, on the other hand, the pyknic type among the manic-depressives (circulars), the cycloids and cyclothymes. The next step was the combination of these groups with criminal types. During the last twelve years there have been made, especially in Germany and Austria, many researches with the object of discovering whether the types of Kretschmer are of any significance as regards Criminology. According to the prevailing opinion, these types are really of importance. First, the members of the pyknic type, sociable and good-natured as they are, usually, it is alleged, commit fewer and much less serious crimes than do the other types (especially the athletic) whose characteristics are egotism, eccentricity, reserve, and unsociability.[4] Moreover, the question arose as to the relation of Kretschmer's types to *special groups of crimes*. Here a difficulty exists which has already played its part in the controversies about Lombroso. How far is it possible to classify the conceptions of Criminal Law, such as murder, manslaughter, larceny, etc., according to anthropological or

[1] See p. 122 of the English edition.

[2] See p. 208.

[3] In the later German editions, Kretschmer, in addition to the term "asthenic," uses also the term "leptosome," describing the asthenic type as the extreme form ("Kümmerform") of the lean leptosome type (see p. 19 of the 9–10 German edition, 1931).

[4] See, e.g. E. Mezger, *Kriminalpolitik*, pp. 93 *et seq.*; von Rohden, *Monatsschrift für Kriminalpsychologie*, etc., vol. xxiv, p. 82; Riedl, *Monatsschrift*, vol. xxiii, pp. 473 *et seq.*; moreover, several contributions to the *Mitteilungen der kriminalbiologischen Gesellschaft* and to the *HdK.* as, e.g. Aschaffenburg, *HdK.* i, p. 835; Gruhle i, pp. 559 and 910; Birnbaum, ii, p. 271.

biological types? Among others, already Franz von Liszt[1] had rightly pointed out that it is impossible to regard the articles of the Penal Code as corresponding to anthropological types. Bleuler and others considered this objection as erroneous, since Lombroso had not aimed at the legal conceptions of crime and the criminal, but at the social ones which, it was urged, remain always the same. "Every sane person who violates the existing social order is a criminal. This conception of crime never changes."[2] Surely, it is self-evident that such an invariable sociological conception cannot exist. Not even Lombroso failed to recognize that crime in the sociological sense changes its contents just as much as does the legal conception.[3] Nevertheless, it is certainly true that the criminological types should not be based upon legal conceptions which take into account, instead of the personality of the wrongdoer, only the single crime in question. This does not mean that Criminology may safely overstep all the limits set by the definitions of Criminal Law by treating a person as a criminal, although he has committed no crime.[4] But within the scope of those actions that are punishable according to Criminal Law, Criminology must be free to establish classifications of its own. It is obvious that the legal distinctions between murder and manslaughter, between larceny and embezzlement, are unsatisfactory from the criminological point of view. The sexual murderer has usually more in common with other types of sexual offenders than with the political murderer who, on his part, may resemble the man who commits high treason. The legal conceptions of larceny, false pretences, etc., cover greatly varying criminological types.[5]

[1] *Strafrechtliche Aufsätze und Vorträge*, i, p. 309.

[2] Bleuler, *Der geborene Verbrecher*, p. 36.

[3] See, e.g., *Crime, its Causes and Remedies* (American edition), pp. 57, 384, etc.; *L'Uomo delinquente* (German edition), vol. ii. p. 278.

[4] See Sutherland, *Principles*, p. 19. From the above it does not follow that—as Dr. Grace W. Pailthorpe writes in her excellent *Studies in the Psychology of Delinquency* (1932), p. 11—sociologically a man is not a criminal until the arm of the law is stretched out to take him.

[5] See, e.g., Willemse, *op. cit.*, p. 24.

These facts have not always been duly considered by Lombroso. On the one hand, it is true, his differentiation of born criminals, criminals by passion, occasional, habitual, juridical criminals and criminaloids forced him to take into account many aspects that were foreign to the law.[1] Often, however, he contented himself with the legal classifications as murderers, thieves,[2] or even spoke simply of " criminals " in abstracto. Modern Criminology tries to avoid such mistakes.[3] Moreover, it is recognized that the tripartition asthenic, athletic, and pyknic, does not cover the great variety of existing bodily types, so that it becomes necessary to introduce mixed forms. According to recent investigations carried out by members of the Crimino-biological School, it would appear that swindlers, for instance, belong mainly to the leptosome or kindred groups: among 300 swindlers examined by Riedl,[4] there were 124 = 41·3 per cent. pure leptosomes, 34 = 11·3 per cent. leptosome athletics, and 18 = 6 per cent. leptosome pyknics, as contrasted with only 36 = 12 per cent. pure athletics and 50 = 16·6 per cent. pure pyknics. Among 200 prisoners convicted of malicious wounding, however, there were only 32 = 16 per cent. pure leptosomes, 4 = 2 per cent. leptosome pyknics, and 7 = 3·5 per cent. pure pyknics, as compared with 87 = 43·5 per cent. pure athletics, 40 = 20 per cent. leptosome athletics and 30 = 15 per cent. pyknic athletics. Among 300 thieves there were 105 = 35 per cent. pure leptosomes, 37 = 12·3 per cent. leptosome athletics, 9 = 3 per cent. leptosome pyknics, 74 = 24·7 per cent. athletics, 43 = 14·3 per cent.

[1] *L'Uomo delinquente* (German edition), ii, p. 266: some forms of larceny belong to the group of habitual crimes, others do not. Or ii, p. 302: the frequency of tattooing varies among the different groups of thieves.

[2] "The lips of violators of women and murderers are fleshy, swollen and protruding as in negroes. Swindlers have thin, straight lips " (Gina Lombroso Ferrero, *The Criminal Man*, p. 16). Or see Lombroso's measurements of skulls (*L'Uomo deliquente*, i, p. 138).

[3] See, e.g., Dr. M. Riedl, *Monatsschrift für Kriminalpsychologie*, etc., vol. xxiii (1932), pp. 473 *et seq.*

[4] Riedl counts among the " swindlers " prisoners with at least three convictions of false pretences (taken in the widest meaning).

pyknics. Expressed more simply by halving the figures for the mixed forms, it would show:

(*a*) *Swindlers:* 50 per cent. leptosomes, 24 per cent. athletics, 26 per cent. pyknics.

(*b*) *Men convicted of malicious wounding:* 27 per cent. leptosomes, 61 per cent. athletics,[1] 12 per cent. pyknics.

(*c*) *Thieves:* 42·7 per cent. leptosomes, 36·2 per cent. athletics, 21·1 per cent. pyknics.[2]

Riedl gives no figures as to the prevalence of the three main types among the general population. According to von Rohden, the figures for the latter are: leptosomes, 60 per cent.; athletics, 30 per cent.; pyknics, 10 per cent.[3] If this be true, the figures reached by Riedl would not prove that the percentage of pyknics is strikingly low among criminals, as is claimed by the Crimino-biological School. There are other contradictions, too. Whilst Mezger states that among cyclothymes crimes of passion are comparatively frequent,[4] Riedl's investigations show the lowest figures for malicious wounding just among the pyknics (from whom the cyclothymes are recruited). It appears that too much importance has still been attached to the legal classification of the crime, instead of to the particular methods of its performance.[5] Riedl's result, for example, that swindlers usually belong to the leptosome group is true at best with regard to particular methods of swindling, whilst other methods that demand

[1] Riedl, p. 475, emphasizes that this high percentage cannot be explained merely by the stronger musculature of the athletics.

[2] It is interesting to compare these figures with certain statements made by Lombroso, *L'Uomo delinquente*, i, p. 213, and by Goring, *op. cit.*, p. 200, fn. 2: "A stout, strong, healthy, thickset individual, if anything rather below the average stature of his class: this is the typical portrait of a person prone to commit criminal violence."

[3] See Mezger, *Kriminalpolitik*, p. 96. For Central Germany, the following figures are mentioned: Leptosomes, 50 per cent., athletics, 30 per cent., pyknics, 20 per cent. (see Mezger, p. 97).

[4] Similarly von Rohden, *Monatsschrift*, vol. xxiv, p. 83; Willemse, *op. cit.*, p. 157.

[5] This is rightly stressed by Willemse, *op. cit.*, pp. 76, 87 *et seq.*, who gives interesting examples of characteristic methods of thefts by leptosome criminals.

sociability and a behaviour calculated to inspire confidence are much more suitable for the pyknic type.

Another important statement frequently made by members of the Crimino-biological School is to the effect that criminals of the pyknic type usually are *occasional* and *corrigible* criminals, whilst among the leptosomes and especially among the athletics are found more *habitual* and *incorrigible* offenders.[1] One of the champions of this School, Viernstein, makes very sweeping statements in this respect: 58 per cent. of the schizothyme prisoners examined by him, he alleges, are incorrigible, whilst the corresponding figure for cyclothymes is only 12 per cent.[2]

In order to recognize the practical consequences of these statements, one must bear in mind that it has always been the explicit object of the Crimino-biological School to eliminate from the Progressive Stage System and other reformatory measures those prisoners who are regarded as incorrigible criminals.

The figures reached by Viernstein are, thus, far more pessimistic than the percentage claimed by Lombroso for his born-criminal type; but their reliability is probably of a similar character. It is, however, only fair to mention the fact that even among the members of the Crimino-biological School considerable objections have been raised against Viernstein's attitude.[3] Above all, it has been urged that the sociological aspects of the problem should duly be examined. It seems, moreover, to be evident that the above reported

[1] Von Rohden, *op. cit.*, pp. 86 *et seq.*; Mezger, p. 96.

[2] According to the same source, 40 per cent. of the prisoners who committed crimes against property were corrigible, the corresponding figures for prisoners who had committed acts of violence and sexual offences being 84 per cent. and 72 per cent. respectively (see Hoffmann, *Monatsschrift*, vol. xxiii, p. 385). These figures seem to be inconsistent with Riedl's statement that crimes of violence are committed mainly by athletics, combined with the thesis that among athletics there are more incorrigibles than among the other goups.

[3] See, e.g., the considered opinion of Sieverts, *Monatsschrift*, vol. xxiii, pp. 588 *et seq.*; vol. xxiv, pp. 107 *et seq.* It is, moreover, the "sceptic among German psychiatrists," Prof. Gruhle (Heidelberg), who has often asked for a more cautious attitude (see, e.g., *HdK.*, i, pp. 200 and 910).

doctrines are at present nothing more than generalizations of certain facts which apply only to special districts and special groups of the population. Many of the investigations hitherto carried out by members of the Crimino-biological School are concerned with inmates of Bavarian and Austrian prisons. That may explain, at least partly, the preponderance of the athletic type. Researches made among the prison population of Berlin, for instance, might perhaps have shown opposite results.

Another branch of Crimino-biological research that is closely connected with the fundamental ideas of Lombroso seems also to require a more critical attitude: the criminological investigations made among *twins*. Johannes Lange's already quoted *Crime as Destiny*, it must be admitted, contains several instructive descriptions of criminal careers. It is, nevertheless, surprising that this book—obviously in a much more dogmatical sense than was intended by the author himself—has been accepted by eminent criminologists as sufficiently conclusive in proving the preponderant significance of the endogenous factors of crime.[1] It is unnecessary here to repeat the objections already brought forward against Lange.[2] Suffice it to say that his cases are numerically much too limited to justify such important conclusions.[3] Moreover, the similarities between most of his monozygotic pairs of twins are, in fact, neither so close nor so significant as may appear at the first glance. Everyone familiar with a very great number of criminal records knows that such careers as those of the Heufelder, Lauterbach, etc., brothers are not extraordinary; their similarity, thus, loses a great deal of its

[1] See, e.g., Aschaffenburg, *HdK.* i, p. 831; Mezger, *Kriminalpolitik*, p. 111; Kinberg, *Basic Principles*, p. 303.

[2] See Sutherland, *Principles of Criminology*, pp. 80–1; Gruhle, *Zeitschrift für die gesamte Strafrechtswissenschaft*, vol. l, pp. 433–4.

[3] Investigations on a wider scale are announced by Rosanoff, *Journal of Criminal Law and Criminology*, vol. xxiv, p. 923. The researches carried out by the Russian Medico-biological Institute in Moscow (see S. G. Levit in *Character and Personality*, vol. iii, No. 3, March 1935, pp. 188 *et seq.*) are apparently not concerned with criminological problems as such.

significance. In fact, there are to be found even important differences among many of the monozygotic pairs,[1] and several of the congruent traits are explainable by environmental influences.

If we attempt to sum up the main resemblances and contrasts existing between Lombroso and the Crimino-biological School, we may perhaps use the following formulations. Both parties aim at the discovery of criminal types, and both are inclined to stress the influence of internal factors of crime; they over-estimate, therefore, the percentage of incorrigibility. The majority of the members of the Crimino-biological School, however, warned by Lombroso's mistakes, content themselves with the allegation of certain inborn tendencies and predispositions towards criminal attitudes.[2] It is in this sense only that they acknowledge the existence of a born-criminal type, not, however, as an anthropological, but as a biological or even as a mental type.[3] They try to improve upon Lombroso's inadequate classifications by the addition of several new groups—a method which, unavoidable as it is, may finally result in superseding every kind of classification.[4] They emphasize the many-sidedness of the "biological" outlook as compared with Lombroso's one-sidedness (which, however, as already mentioned, was by no means so extreme as is often alleged). Bodily features are significant only so far as they are connected with corresponding mental traits, and social factors are at least not entirely overlooked. Even among the different branches of the Crimino-biological School, it is admitted, there is to be found

[1] Willemse, *op. cit.*, pp. 162–3, presumes that one of the Heufelder twins belonged to the pyknic, the other to the athletic type.

[2] Lenz calls it "*kriminogene Disposition*" (*HdK.*, ii, p. 62; *Grundriss der Kriminalbiologie* p. 9, etc.).

[3] Von Rohden, *op. cit.*, p. 77; Gruhle, *HdK.*, i, p. 911 (characterological-sociological type).

[4] Such a negative attitude is strongly supported by Saldaña, *Journal of Criminal Law and Criminology*, vol. xxiv, pp. 333 *et seq.*, and, according to him, by the Portuguese criminologist Mendes Correa. See, moreover, Saldaña's Biotipologia criminale in *La Giustizia Penale*, 1934, Parte Prima, pp. 326–374.

considerable variation of opinion and method.[1] The Belgian[2] and Latvian scheme, for instance, is mainly concerned with the psychiatric part of the problem, the Bavarian more with the biological. It is obvious that these differences are closely connected with the fundamental outlook as regards crime and punishment that is dominating in the various countries. It is almost common opinion that Russian criminologists[3] represent the most sociologically minded and anti-Lombrosian wing; Bavarian, Austrian, and Italian criminologists the most biologically minded section. Kinberg[4] points out that the European School of Criminology, as a whole, is tending more towards the individual, endogenous theory of criminality, whilst Northern American criminologists are more interested in social aspects, and he finds the explanation of this contrast in the differences of social history and present social conditions. There is certainly some truth in this statement, and the example of Russia furnishes only a further corroboration of the rule. Nevertheless, it cannot be overlooked that, even apart from Russia, European Criminology is by no means unanimous. It would have been astonishing indeed if the occurrences of the Great War and of the post-war period had not brought home to European criminologists the importance of social factors. If we learn, for instance, that in Germany the index of criminality (calculated per 100,000 of the population) was as follows:[5]

(*a*) For petty theft, in 1913, 210; in 1920, 428; in 1923,[6] 633; in 1930, 156.

[1] See the survey given by Mezger, *Der Gerichtssaal*, vol. ciii, and *Kriminalpolitik*, pp. 111 *et seq.*

[2] As to the Belgian system, see the report by its eminent exponent, Dr. Louis Vervaeck, *Journal of Criminal Law and Criminology*, vol. xxiv, pp. 198 *et seq.*

[3] As to the attitude of the Criminological Institute in Moscow, see Kamenetzki, *Zeitschrift für die gesamte Strafrechtswissenschaft*, vol. li, pp. 597 *et seq.* A review in *La Giustizia Penale*, 1934, Parte Prima, p. 84, speaks of an " anti-lombrosissimo " attitude of the reviewed Russian writer.

[4] *Basic Principles of Criminology*, p. 167.

[5] The following figures are taken from Roesner, *HdK.*, ii, pp. 44–5.

[6] I.e. when inflation had reached its highest level.

(*b*) For receiving of stolen goods: in 1913, 21; in 1920, 76; in 1923, 136; in 1930, 18.

(*c*) For murder: in 1913, 0·23; in 1920, 0·44; in 1923, 0·29; in 1930, 0·18.

(*d*) For malicious wounding: in 1913, 139; in 1920, 105; in 1923, 84; in 1930, 99.

It seems really impossible to explain such figures exclusively or even mainly on biological or pathological lines or by changes in law and procedure only.

On the other hand, the tendency to stress the importance of inborn factors is not entirely strange to modern American Criminology. In addition to what is called the "Neo-Lombrosian theory" which replaces the physical type by a psychopathological one,[1] the doctrines of the endocrinologists are to be mentioned in this connexion. One cannot regard them, as Saldaña does,[2] simply as a refutation of Lombroso. They are, of course, opposed to Lombroso's exaggerated views upon the influence of heredity.[3] But they are not averse to his explanation of crime as mainly the result of other internal factors. According to the theory of the endocrinologists, the criminal individual who suffers from serious disturbances of the ductless glands is the typical born criminal.[4] The obvious advantage of this theory, as compared with the hereditarian doctrine, is that it is a more suitable explanation of the fact that so often only one child (or one pair of twins) is a criminal, whilst the other descendants are not.[5] On the whole, however, the endocrinological view shows shortcomings at least equivalent to those of the other successors of Lombroso.[6] When Schlapp and Smith, for instance,

[1] Sutherland, *Principles*, pp. 94, *et seq.*

[2] *Journal of Criminal Law and Criminology*, vol. xxiv.

[3] See, e.g., Lombroso, *Crime, its Causes and Remedies*, pp. 151, 174, 223, 376, and Parmelee's Introduction to the American edition, p. 31. As to the whole problem see M. Ginsberg, *Studies in Sociology* (1932): Inheritance of Mental Characters.

[4] See Schlapp and Smith, *The New Criminology* (1928). The third chapter of this book bears the title: "Back to Lombroso."

[5] Many of Lange's cases belong in this connexion.

[6] See the remarks by Sheldon and Eleanor Glueck, One Thousand Juvenile Delinquents (1934), p. 283.

try to explain the criminal career of a girl by the hypothesis that she "probably" suffered from a glandular defect from birth,[1] it is obvious that one might ascribe with equal justification the girl's downfall to the untimely loss of her parents. In fact, it may probably be advisable to combine both factors and to dismiss finally all those theories that limit their considerations to one single side of the problem, instead of following the model set by Burt, Healy, the Gluecks, and others.

Only a few remarks need be added on the *penological* views of Lombroso and their relation to modern Penology. Anthropological and hereditarian theories of crime bear always a somewhat negative character. Nevertheless, it is a strange mistake to believe that Lombroso's penological programme was sterile and limited to a recommendation of the death penalty or preventive detention for born criminals. It is of little value to dwell upon the numerous lapses he committed also in the field of Penology: his suggestion to render harmless dangerous criminal tendencies by supplying the individual with a profession which would enable him to indulge in his favourite occupations without breaking the law; his struggle against the better education and training of the prisoner, based upon the apprehension that education would promote criminal habits.[2] On the whole, however, he expressed very sound views as to the fundamental principles of reformatory treatment of all prisoners except the born criminals. He was especially proud[3] of the fact that one of the chief founders of the Elmira Reformatory movement, Z. R. Brockway, had confessed that his work had been inspired by Lombroso's ideas.[4] Considering that Elmira was, to a certain extent, the pattern of the English Borstal system,[5] Lombroso may even

[1] *Op. cit.*, p. 213.

[2] As to this problem see: Lombroso, *Crime, its Causes and Remedies*, pp. 105 *et seq.*, and, on the other hand, the excellent remarks in the *American Prison Handbook*, Introduction to the edition of 1933; Sutherland, *Principles*, pp. 452 *et seq.*; Roesner, *HdK.*, ii, pp. 165, *et seq.*; Exner, *HdK.* ii, p. 21; Goring, *op. cit.*, pp. 267, 274, *et seq.*

[3] See Gina Lombroso Ferrero, *The Criminal Man*, p. 192.

[4] Z. R. Brockway, *Fifty Years of Prison Service* (1912), p. 215.

[5] See Sir Evelyn Ruggles Brise, *The English Prison System* (1921), p. 91.

claim the honour of being counted among the originators of this system. Moreover, whilst he was a convinced supporter of probation, he suspected the value of solitary confinement.[1]

These few indications may suffice to show that Lombroso's penological views have much in common with modern ideas. But even with regard to the criminological part of his work, it cannot be maintained that modern Crimino-biology has suddenly emerged as a quite new science out of the complete breakdown of Lombrosianism. We are rather confronted with a gradual process of partial abandonment, partial retention and improvement of his methods and results—a process not infrequently encountered in the development of scientific doctrines.

[1] See his *L'Anthropologie Criminelle*, 2 ed. 1891, pp. 145 *et seq.*

A STUDY OF HISTORY[1]

A REVIEW OF PROFESSOR TOYNBEE'S BOOK

By M. POSTAN

I

TO review Professor Toynbee's three volumes eighteen months after their publication is to run all the risks of delay without reaping any of its benefits. A belated review is always in danger of becoming a mere review of reviews, either a reflection of the earlier opinions or a revulsion from them; and in dealing with Professor Toynbee's book the danger is only heightened by the certainty and unanimity of the world's first opinions. At the same time the interval has done nothing to facilitate the formation of a final judgment. A late commentator is at as great a disadvantage now as the prompt reviewers were a year ago, in that he still has to survey a mere beginning of the great enterprise. Now, as a year ago, little can be said, either in praise or in criticism, that is likely to survive the subsequent unfolding of Professor Toynbee's design. The present essay has therefore nothing to offer except a few reflections, some of which relate to things said about the book rather than in the book itself, and all of which are destined to be provisional.

The main themes of the three volumes are too well known by now to require a detailed retelling. Professor Toynbee sets out to find in history an explanation of the rise and growth of civilizations. Having distinguished civilization from society, and having defined the former as the phase of creative activity in the latter, he then proceeds to explain the appearance of the creative phases. He dismisses the theories of race and geographical environment in their conventional formulation. Whatever his detailed arguments against these hypotheses (and he mostly attacks them on the ground of their relativity and inconsistency), his real, though unex-

[1] A. Toynbee, *A Study of History*, Vols. I.–III. London, 1934.

pressed, objection to them is contained in his definition of civilization. Geography, climate and race, treated as constants, cannot account for something which to the author is a dynamic and transient state. The true cause must be as dynamic as the civilization itself, and this cause Professor Toynbee finds in " challenge and response." Societies are exposed to a variety of " challenges." Some of them come from outside: hard climate, rigours of migration, poverty of the soil. Others rise from within society itself: over-population, penalization of minorities. Some are due to events in the physical world: desiccation, the spread of the forest. Others are due to human agencies: the pressure of neighbouring societies on the marches, the assimilating action of foreign cultures. But whatever its nature and origin, the challenge makes the existence of society on pre-existing terms impossible. The society must either decline and disappear or accept the challenge and adapt itself to the new conditions; and it is in accepting the challenge and in the process of responding to it that societies generate that intense form of activity which distinguishes the creative state of civilization from the passive state of mere social existence. Yet not every response to every challenge will lead a society to a civilization. Challenges can be so severe and the responses they produce so exacting that the whole of social activity is absorbed in re-establishing and maintaining social existence. In order to generate a state of true civilization the challenge must be moderate; neither too hard nor too easy. The " Golden Mean " is the rule of the truly effective challenge and of the truly creative response.

Having thus accounted for the rise of civilizations, Professor Toynbee proceeds to the analysis of their growth. Growth to him is a discontinuous process, a sequence of disequilibria and states of balance; of old problems solved and of new problems rising on the ruins of old solutions. Yet within this series of alternating phases there is a constant progression and a direction. The reciprocal motions of a growing civilization

all lead to higher forms of life and culture. In their upward climb civilizations achieve an ever greater command over environment and attain ever higher degrees of differentiation (which presumably means variety and complexity of social forms), and of etherealization (which presumably means freedom from preoccupation with the material facts of existence). For this ascent the existence of leading men and leading societies is essential, and the mechanism of leadership, like growth itself, is a process of alternating phases, of " withdrawal and return." Leading societies and leading men must pass through a phase of inactivity before they are able to place themselves ahead of the others on the way to the higher levels of civilization.

These main themes, and especially that of " challenge and response," dominate the whole of Professor Toynbee's enormous collection of evidence. The ingenuity with which they are fitted to disparate facts, drawn from an infinite variety of epochs and places, the insight and imagination which transform the detached and seemingly irrelevant facts into essential parts of a unified theory, all make the book seem unlike anything else ever written about history in the English language. Over and over again the argument is brought to the very verge of a great revelation, and over and over again the reader is made to feel that he is hot on the trail of the one and the only meaning of history.

But the chief value and the chief fascination of the book is not so much in the great expectations which its principal ideas raise, as in the facts from which they rise. The infinite wealth of fact and allusion creates a pattern as intricate and a texture as rich as those of an Oriental carpet. There is no subject in history which it does not touch, and for every subject it touches it gives a complete summary of the present position in scientific discussion. Thus, whether the book fails or succeeds as a philosophy of history it will always remain a concordance of history, an encyclopædia of *Kulturgeschichte* in a non-alphabetical order. Professor Toynbee

possesses the gifts of a truly great encyclopædist, of a Diderot or a D'Alembert, for he is capable of holding and giving out essential information while pursuing an argument. It matters not that on some topics the information is slightly out of focus, or that on others the present state of knowledge is too nebulous and too yielding to be even summarized with impunity by men with a philosophic purpose. What is important is that for the first time the essentials of modern knowledge in every imaginable field of history and archæology have been set out by a man capable of comprehending and handling them.

The book will also delight some people, as it does the reviewer, by its approach to historical evidence. Professor Toynbee does not pursue historical information for its own sake. To him historical information is a species of scientific evidence. Like the data of archæology and anthropology, and in combination with them, the facts of history supply an empirical foundation to the science and philosophy of society. In support of this attitude Professor Toynbee adduces a number of arguments some of which will not carry conviction, but the attitude itself is sufficiently clear and sufficiently reasonable to stand, even if the supporting arguments do not. Thus, to insist that historical facts are comparable even when separated by centuries and millennia, merely because the time covered by the history of civilization is infinitesimally small relatively to the antiquity of mankind, is not very helpful or relevant, since it is not the history of mankind but the history of civilizations that Professor Toynbee has chosen for his subject. The real argument for his view will be found in the title of the chapter in which it is expounded. To have called it "The Philosophical Contemporaneity of the Civilized Societies," is to have produced the best justification. However widely separated in time, historical evidence can be contemporaneous in meaning, i.e. in the light which it is able to throw on the constant and the unchanging elements of human society and human nature.

This attitude Professor Toynbee shares with a large number of younger historians who regard themselves as partners in the joint enterprise of social science; and if allowed to put on his work the simplest and the most familiar of the many interpretations which it permits, they would probably find themselves in agreement not only with his attitude to historical evidence but also with the substance of his historical ideas. Simple-minded social scientists could, if they wanted, discover familiar and acceptable meanings in many of Professor Toynbee's strange-sounding words. At its simplest and most definite, " challenge and response " may be merely a euphonious way of referring to what writers more prosaically minded have been accustomed to describe as " effects of environment." Biologists and sociologists and even the old-fashioned historians have been in the habit of considering the social as well as the geographical environment, and no modern definition of environment is as simple as those Professor Toynbee attacks in his discussion of race and geographical factors. In its modern connotation environment is no more (and unfortunately no less) external to the object than Professor Toynbee's " challenge." Nor is it always considered in a static condition. When historians and sociologists emphasize the effects of environment on the *origins* of phenomena, they cannot help considering it as a factor newly introduced, as a dramatic change and a revolutionary rearrangement of conditions. They treat the political effects of the oversea migration of the Anglo-Saxons, or the social effects of the Eskimo settlement on the Arctic icefield, in a way which differs very little from what Professor Toynbee has to say when he treats the same phenomena as " challenges." And in many other situations which Professor Toynbee classifies as " challenge and response," they would recognise the old and familiar " effects of a changing environment."

There is a similarly familiar ring about his other inventions. His " Golden Mean," when taken in its most sensible version,

reminds one very closely of the nineteenth-century discussion of the blessings of the British climate. Professor Toynbee, like Buckle, believes in the advantages of an environment which is neither too easy nor too hard; and like Buckle he gives no other indication for identifying and locating the golden mean except as the point at which the best results occur. But however circular, the argument is familiar and simple. Equally familiar is the idea underlying Professor Toynbee's notion of growth. If taken in the simplest and the most definite of its possible meanings, it becomes yet another version of what Marxists describe as the dialectical process. Professor Toynbee compares evolution to the mechanics of walking: a mode of progression in which the body is thrown out of balance after each successive state of equipoise. The simile and the underlying concept of advance through antithetic stages would be hailed by the Leninists and Stalinists as the nearest to their own simplified rendering of Hegelian dialectic. It is somewhat more difficult to get a definite meaning out of the " withdrawal and return," but in so far as it exists it is exceedingly plain. In one of the senses in which Professor Toynbee employs it, it appears to be an additional application of the dialectical concept. " It is sown in dishonour, it is raised in glory; it is sown in weakness, it is raised in power." But sometimes the author uses it to indicate something which is simpler still. Many of his examples merely demonstrate that societies and men sometimes gather forces before starting a successful enterprise, or that men must collect their thoughts before giving them out to the world. This simplest of truths is all that can be derived from his discussion of Italy's greatness in the sixteenth century after her relative insignificance in the thirteenth and the fourteenth, or of England's greatness in the nineteenth century after several centuries of relative obscurity. Neither Italy in the thirteenth century, nor England in the seventeenth, were in any real sense of the word inactive, and all that the word " withdrawal "

can mean here is that the earlier record was overshadowed by the subsequent achievements. Nor would anybody in his senses, even a child, deny the suggestion that periods of exceptional achievements are apt to be preceded by periods of which the achievements were less exceptional.

II

Thus, if all Professor Toynbee's ideas were presented and accepted in their simplest and most definite meanings, the book would, in addition to being a great encyclopædia, become also a great sociological anthology: a magnificent collection of beautifully reproduced illustrations of the familiar ideas about the rise and growth of civilizations. But great as is our need of such a collection, and remarkable as are the gifts which Professor Toynbee brings to its selection and arrangement, there is not the slightest probability of the book being offered or read as a mere anthology of social history. It is not through accident and certainly not through ignorance that Professor Toynbee has rejected the traditional and familiar terms. He has coined new words because he has been seeking new concepts. By his new phraseology he has intended to convey a range of new meanings and a system of new ideas. It is this intention that will probably raise most doubt, for the bulk of his new inventions are too wide and too vague to lead to a true sociological discovery, while the few which happen to provide openings for a truly original and a perfectly definite enterprise are left unexplored. The width and the vague suggestiveness of most of the book's concepts are among its most conspicuous features. Over and over again the author imparts to his terms a multiplicity of new meanings by depriving them of their old clarity. A system of ideas is thus reared, which by its very vagueness offers more than it can give and excites reason beyond its power of satisfying it. And what emerges is not a new philosophy of history but a multiplicity of old philosophies muffled and veiled.

The reason why Professor Toynbee has been led to charge most of his concepts with so many vague meanings will be found in his technique of argument. The technique is that of literary metaphor. Whenever he approaches a new and a complicated concept he suddenly abandons the ordinary procedure of analysis and turns to artistic images. "Let us shut our eyes for the moment to the formulæ of science and open our ears to the language of mythology" (vol. I, p. 271); and in the language of mythology we get the contrasting images of Zeus and Prometheus, instead of an analysis and a definition of the difference between reactionary domination and creative initiative. The Chinese symbols of the Yin state and the Yang state are employed to convey the distinction between society and civilization, the simile of walking or rock-climbing to describe growth and evolution. These visual images are among the most fascinating features of the book, importing into the argument brilliant flashes of poetic vision, which appear to penetrate deeper than the dim light of mere sense. Had he used them to illustrate concepts previously defined, they would have added to the book's charm without detracting from its intellectual quality. Unfortunately, they are often used not as embellishments, but as structural parts of the arguments; not as aids to exposition, but as substitutes for thought. He reaches out for an image, not after he has succeeded in building up a logical concept, but when he is afraid that logic may fail him. "The event can best be described in these mythological images, because they are not embarrassed by the contradiction which arises when the statement is translated into logical terms" (vol. I, p. 278). Unfortunately concrete poetic images are only too good a refuge from the embarrassments of sense. A whole regiment of loosely related meanings can camp at ease in the generous shade of a single metaphor. The author can use them all at once or one at a time, to suit the argument and to fit the example; and however scrupulous and honest his intentions (and

Professor Toynbee's are irreproachable), his achievements are bound to suffer from this excess of freedom.

It is this dangerous licence of pictorial image that is responsible for the reader's difficulties with Professor Toynbee's notions of society and civilization. By a cumulative effect of simile and allusion, Professor Toynbee succeeds in creating the impression of a civilization as an active phase in the life of society, but as long as this remains a mere impression, with a scope and a meaning undefined, it affords the author a fatal freedom of operation. It enables him to employ the concept in several conflicting senses to serve the passing contingencies of his argument. Thus, if civilization means an active phase in the life of a society it cannot be exposed to challenge, transferred (vol. I, p. 334), or raise its head (vol. II, p. 79). And if civilization is activity, is it identical with the activity of responding to a challenge or is it something generated in excess of the response? And if civilization is defined as the activity of responding to a challenge, is not then the whole discussion of the challenge and response as the "cause" of civilization somewhat tautological? And if civilization is an activity identical with a successful response, how are civilizations to be ranked, and how can successful responses be compared with each other and distinguished from those which failed? If they are to be judged by the results which they have created, and in which they stand enshrined, civilizations are at once identified with what Professor Toynbee is inclined to describe as "cultures," which he apparently does not, himself, identify with civilizations. His introductory analysis of universal civilizations suggests that he intends to adopt for his index the degree to which a civilization approaches universality. Had he done this he would then have had to decide on the best way of measuring a deviation from the index; but he has saved himself the trouble by refusing to employ any single standard, and by using impartially an infinite variety of measures. He sometimes ranks civilizations accord-

ing to a single cultural achievement, arbitrarily selected; thus we are told that the Icelandic civilization is higher than that of the Scandinavians at home, because the epic is a higher form of art than the drama. Sometimes the index is the capacity for extending over great areas (Russia from the fourteenth to the eighteenth centuries), but sometimes to succeed means only to hold the ground against a powerful political and cultural influence. Sometimes it is the capacity for repelling military aggression (Austria in the seventeenth and eighteenth centuries); sometimes it is the capacity for indulging in it (Turkey until the seventeenth century). In one case to have come to Constantinople as hall porters and commissionaires is described as a successful response to the challenge of penalization (vol. II, p. 214), and in another place readers are left wondering whether the humiliating ceremonies which the Dutch merchants had to undergo in unreformed Japan are also an instance of a Yin-activity, generated by responding to the same challenge. If any common feature at all can be discovered in the infinite variety of Professor Toynbee's successful responses, it is the one which he himself would hardly like to see put into words. A complete and impartial list of Professor Toynbee's "successes" would be merely a list of events and situations which happen to have made sufficient splash to receive large notices in history books. Thus, in spite of the suggestive references to the German *Aufklärung* in vol. III, the one German achievement which he constantly emphasizes is not the art of the sixteenth century or the music and philosophy of the eighteenth, but the formation of the Empire in the nineteenth. Similarly, the great period of the history of Russian civilization is not her great renaissance of the nineteenth century, but her conquest of the Eurasian continent in the sixteenth, seventeenth and eighteenth. The achievement of Polish civilization is not the literary and intellectual activity of her post-partition days, but the battle of Tannenberg in 1410. In the same list medieval Hungary

occupies a more important place than medieval Flanders, Venice appears greater than Florence, and of the fair land of Languedoc there is hardly a word. In short, if there is any standard at all, it is that of the Ph.D. student, who in his thesis on great men measured greatness by the size of the notices in *Who's Who*. And yet, however superficial and however unintentional, this standard is the only one by which Professor Toynbee's civilization can be appraised as an " activity " distinct either from its " field," i.e. the society, or from its achievement, i.e. culture, social and political organization and mode of life.

Still greater are the penalties which Professor Toynbee pays for the convenient vagueness of his concept of " withdrawal." So many and so different are the suggestions which the image of withdrawal carries with it that it enables and leads him to group together most ill-assorted situations. Solon becoming a merchant before returning to his native city as a law-giver, and Hamlet immersed in introspection, are both instances of withdrawal preceding a creative effort. So also is Peter the Great's journey to the West, Machiavelli's enforced retirement into a provincial town, St. Paul's anti-Christian prelude, England's lack of international ties from the fifteenth to the eighteenth centuries (*sic!*), the psychological condition of puberty, Hindenburg's retirement from active service a few years before the war and again before his election to the presidency.

III

Professor Toynbee has utilized to the full the dangerous possibilities of his poetic method and yet he has failed to avail himself of its one and only opening for a truly scientific analysis. In one concept at least, that of " challenge and response," the widened meaning which a literary image provides could easily have involved Professor Toynbee in a sociological enquiry. The very binomial structure of the

term, the coupling of the active agent with the effects of its action, might have enabled Professor Toynbee to proceed much farther than the biologists and sociologists have so far done in abolishing the artificial abstraction of the object from the environment. The possibility has not escaped the author and is implied in his treatment of concrete examples. Forest and marshes, steppes and rivers, migration over seas and mountains, all acted both as stimuli and as soporifics in the lives of different societies. What a geographical factor or a political event turns out to be in the history of a society depends, as Professor Toynbee himself admits, on the society itself. " Transmarine migration is merely a possible stimulus and not an automatic and infallible forcing process of mental growth, and if the stimulus provokes no response, there is little virtue in the stimulus " (vol. III, p. 135). Thus, had he responded to the challenge of his own terminology, he would have tried to bring out the factors that make a challenge challenging. He would have given us the analysis of societies challenged and would have tried to find by what social rearrangements, by what changes in their material and spiritual equipment, some societies were made to feel a certain phenomenon as a moderate challenge, while others were either not challenged by it at all, or challenged only too severely. He would also have classified responses as well as challenges, and would have analysed the mechanism of response. Why did the Assyrians fail to respond to the Scythian challenge while the Medeans succeeded? Why was the English response to the Scandinavian challenge different from that of the Irish? Why did the Zoroastrian and the Jewish societies fail while the Mohammedans succeeded in meeting the Hellenic challenge? How does the capacity for experiencing a challenge differ from the ability to respond to it successfully? Do the same social factors which determine the degree of the challenge also determine the quality of the response?

All these questions may still be answered in the subsequent

volumes, and if they are, Professor Toynbee's work will automatically become a contribution to social science. The problems of challenge and response, which it so far leaves unsolved, are all sociological problems. They are sociological not only because they concern the internal structure of society, its social organization and social equipment. Even in his first three volumes, Professor Toynbee occasionally raises issues which belong to the inner structure of societies (e.g. his " stimulus of penalization " or the social penalty of " excessive responses " in Turkey and Sparta), without, at the same time, turning into a scientific sociologist. What makes a question sociological is not only the nature of the problem it raises, but also the nature of the answer it requires. It is the great virtue of some questions that they can only be answered in a sociological way, by the laborious and painful process of social analysis, by defining and classifying social groups and institutions, by counting and measuring the differences in social arrangements from place to place and from time to time.

Sociological treatment also involves a difference of scale. There is no objection to the scientific analysis being extended to the whole area of Professor Toynbee's field. The only objection to his scale is that it is beyond the powers of any human being, however gifted and learned. For some parts of the field the information is so scanty and so unreliable that close analysis is impossible and for those parts of the field where facts are sufficient they are too numerous to allow any time for excursion into other areas. The scale of the social scientist is therefore infinitesimally small compared with that of Professor Toynbee's book. His subjects are necessarily microcosmic. He does not intend to solve the problems of civilization and society by a frontal attack on the massed evidence of all the historical societies. All he hopes to do is so to organize the study of his minute topic as to be able to answer by the light of its evidence at least some of the problems which are common to society in general. In this

he differs not only from the antiquarian, who is interested in his patch but has no questions to ask, but also from the philosophical historian, like Professor Toynbee, who has all the questions in the world to ask, but no patch on which even a single satisfactory answer can be raised.

There is no reason why Professor Toynbee should have written a work in sociology or social history. He has produced a great encyclopædia, a brilliant anthology, and an essay in philosophic history. But until he writes a work of sociology he will not be able to rid himself of his vague images or to meet the challenge of his fruitful ones.

REMARKS ON METHODOLOGY OF THE SOCIAL SCIENCES

By FELIX KAUFMANN

§ 1. HOWEVER many contradictory definitions of philosophy may exist, there can hardly be any doubt that the essence of philosophizing consists in thoroughgoing reflection. Thought must be made clear and distinct, implicit assumptions made explicit. Such a type of thoroughgoing reflection, going back to the fundamentals of knowledge, is clearly necessary for scientific progress, especially in those phases of development where new facts are discovered which cannot be satisfactorily fitted into the traditional system of knowledge. One of the most illuminating examples of this is provided by Einstein's reflections on the meaning of contemporaneousness of events at different points in space. These were induced by the surprising results of Michelson's experiment, which were hard to reconcile with Maxwell's theory and which constitute the keystone of the special theory of relativity. But, as was emphasized some years ago by an eminent American physicist, Professor P. W. Bridgman, these reflections, which aim at ascertaining what the physicists really mean when they speak of the contemporaneousness of events distant in space from one another, might have been arrived at quite independently of Michelson's experiment; and had this in fact been the case, the physicists would have been spared much confusion. Bridgman stresses the necessity of operational thinking in physics: physical concepts must be defined rigidly in accordance with the meaning they actually have in scientific procedure. The significance of this requirement is most clearly brought out by the recent developments in physics (quantum theory), and to-day it is accepted without reservation—although not always as Bridgman formulated it—by the overwhelming majority of leading physicists.

Among social scientists, on the other hand, there is less general agreement on this point: a situation which it is often attempted to justify by asserting a fundamental difference between the methods of natural and of social science respectively. The natural sciences, or rather the sciences of inanimate nature, according to this argument, are sciences dealing with a realm subject to strict quantitative laws. Causal necessity reigns there, and with it, unique determination, which makes possible the precise definition of scientific concepts. The social sciences are, on the other hand, not concerned with the realm of necessity, but with a realm in which at least partial freedom exists, which has its origin in the freedom of the will, so that here precision cannot be attained, and ought not to be insisted on. Rational knowledge is regarded as stopping at the threshold of mental life, and perhaps, indeed, of all life as such.

But more careful consideration shows that this view, although it contains elements deserving of notice, needs to be radically revised. In the first place, it must be asserted that it is based on false assumptions regarding the character of natural laws. The supposed "rigid," "inviolable," "necessary" validity which is ascribed to them—a conception which we cannot examine here—is merely a prejudice; it is a vestige of the original anthropomorphic conception of laws as norms, superimposed upon natural events. This point has already been conclusively answered by Hume's critique of the concept of causality—a critique which, contrary to the view still commonly held, was not refuted by Kant—and, moreover, the latest developments in physics have completely undermined the idea of strict causality from the side of the natural sciences themselves. When we realize, in addition, that no mathematical judgment as such asserts anything about reality, and that therefore the necessary (because analytical) validity of mathematical propositions cannot be ascribed to the propositions of the mathematico-natural sciences, which have only empirical validity, then it becomes

clear that the customary distinction between the " strict laws " of the natural sciences and the " mere rules " or " tendencies," which are supposed to be yielded by the social sciences, cannot be sustained in any rigid form. We shall speak later of the methodological problem which arises from this analysis. Here it is sufficient to bear in mind that the contrast between exact and inexact empirical propositions is not an absolute one.

Quite apart, however, from the inadequacy of the argument we have just criticized, it appears that the supposed connexion between it and the conclusion drawn from it does not hold good. Because a general statement (a rule) exhibits an inadequate degree of exactness, so that therefore no very reliable predictions as to actual occurrences can be made from it, it does not follow that such a statement cannot be clearly and unequivocally formulated. An incorrect statement need not be less clearly formulated than a correct statement; a supposition or a doubt can be expressed just as clearly as a firm conviction, a set of alternative hypotheses just as clearly as a blank assertion.

For a statement is clear when the conditions of its truth, i.e. the criteria upon which its truth or falseness are supposed to depend, are precisely stated; but the nature of these conditions may vary indefinitely, and the question whether they are in fact fulfilled in a given case is a quite separate one.

Once we have laid down the possibility of clarifying sociological statements, we need not waste many words on the question of its desirability. Consideration of half a century of violent and interminable methodological controversies, resulting mainly from the misunderstanding of the opponent's thesis, suffices to show the urgent necessity for such clarification.

It is no use trying to avoid all methodological disputes by pretending that one's method is dictated by the " bare facts." Even such a simple statement as that in a given space-time situation, a given quality, e.g. blue, is to be found, involves interpretation. It must be fitted into a complex of experiences,

since it presupposes a knowledge of what " blue " is; this is valid *a fortiori* for the far more complicated structures of social facts. Even the basic hypothesis of all sociological thinking, according to which certain physical phenomena must be interpreted as actions, i.e. as the effect of psychological acts of will, is bound up with an enormous complex of previous experiences. There is, therefore, no thinking which does not involve presuppositions, and sociologists who claim to base themselves upon bare facts, in reality base themselves on implicit assumptions—a misunderstanding which gives rise to much confusion in scientific work.

Granted, then, the theoretical possibility of logically clarifying sociological statements, and also its desirability from the standpoint of the aims of knowledge, we have now to consider, taking into account the present state of knowledge, whether such an enterprise is likely to be successful. This question must also be answered in the affirmative. For one of the most serious obstacles to its success was to be found in misconceptions concerning either philosophical thought in general, or the methods of other sciences, to which the social sciences were supposed to be related either by analogy or contrast. Thus there arose various unfounded views, which made the contrasts between competing sociological methods appear rigid and immutable. These misconceptions have, however, been progressively cleared up in the last few decades, and to-day they can be regarded as for the most part disposed of.

A considerable service is rendered towards the clarification of sociological problems if these misconceptions are systematically collated, shown to be invalid, and their relevance to the methodological questions of sociology explained.[1] For practical purposes the following grouping of misconceptions can be adopted; but group (*a*) cannot be sharply separated from the other groups.

[1] Cf. Kaufmann, *Die Methodenlehre der Sozialwissenschaften*, 1936 (published by Julius Springer, Vienna).

(*a*) Misconceptions concerning philosophical thought in the narrower sense of the word.

(*b*) Those relating to mathematical and logical thought.

(*c*) Those relating to natural laws in general.

(*d*) Those concerning the problems of life (biology).

(*e*) Those concerning the problems of mental life (psychology).

(*f*) Those concerning the problems of the knowledge of values (axiology).

(*g*) Those which are connected with the historical aspect of the social sciences.

§ 2. We will touch quite briefly upon some of the most important consequences connected with the methodological problems of the social sciences. The hope of being able to make statements about events of historico-social reality which are *a priori* valid, and so cannot be refuted by experience, is deceptive. The logico-mathematical propositions, generally selected to illustrate the possibility of *a priori* knowledge, have absolute validity only because, or in so far as, they make no assertions about reality. Einstein's famous dictum, " In so far as the propositions of mathematics refer to reality, they are not secure, and in so far as they are secure, they do not apply to reality," [1] puts this point incisively. Further, the assumption that we can obtain apodictically certain sociological statements, even with the aid of a special kind of " understanding," is shown to be untenable.

The fact that I at once grasp the " meaning " of a certain mode of behaviour on the part of another person is indeed in the overwhelming majority of cases a useful symptom or indication that my interpretation is correct. But it is no *criterion* of this correctness. The only possible criterion is whether the interpretation turns out to be correct, i.e. whether the assumption holds in fact. When therefore certain statements with regard to social reality within the framework of a given line of research are accepted as valid without ques-

[1] *Geometrie und Erfahrung*, Berlin, 1921, p. 3.

tion, this can only mean that they are not to be subjected to verification now, and that data which cannot be reconciled with them are not to be taken into account—in such cases we speak of conventions (in the theoretical sense)—but it cannot mean that verification is to be excluded *a priori*. It follows, further, that no method of research can be declared *a priori* to be the only adequate one. Thus it becomes all the more necessary to ascertain the scientific reasons which lead to one method being preferred to another.

Scientific reasons are those which have reference purely and simply to the aims of knowledge. It is not to be denied that these aims on their part find in the main their origin in practical interests, i.e. emotional needs. But in spite of this the scientific reasons can be isolated from the emotional background, and methodology is only concerned with the former. What has just been said determines our attitude to the doctrines of radical pragmatism, of historism, and of sociologism, which have overlooked or not sufficiently taken into account this point. It is true that the antagonistic attitude of these doctrines to the methodological dogmatism which we have criticized in the last paragraph is understandable, and up to a point quite justified. But they overshoot the mark when, in sympathy with the criticism of those principles for which absolute validity is claimed, they dispute *in toto* the autonomy of knowledge (i.e. the possibility of isolating the aims of knowledge, and of concentrating attention on them) and merely substitute for an arbitrary absolutism and a wrongly understood objectivism a complete relativism and subjectivism. It can be said that all empirical knowledge is relative in so far as it is possible that it may be refuted by later experience, and subjective in so far as it does not consist merely in a passive reception of objectively given " things in themselves " but necessarily implies syntheses of the " material of experience," which latter cannot be thought of in isolation. But this relativism and subjectivism must not be understood to mean that every sociological statement receives its unequi-

vocal meaning only by being related to a special historical background of experience. This fallacy can best be exposed by considering the intimate connexion between the meaning of a judgment and the conditions of its truth. It then becomes clear that the limits of the intersubjectivity of a judgment are those of the ability to ascertain whether these conditions have or have not been fulfilled. In this connexion, one must not be led astray by the fact that in very many cases verbally identical communications will have to be interpreted differently according to who has made them. For interpretation in the ordinary sense is not confined to the information communicated, but covers also the motives or reasons why it was communicated. Interpretation in the wider as opposed to the narrower sense presupposes generally an intimate knowledge of the speaker, his personal characteristics, and his environment.

We must reject, accordingly, both the denial that any sociological method can lead to objective knowledge of social phenomena, and also the extravagant claims put forward on behalf of *a priori* methods. The real problem is, while carefully avoiding both dogmatic and relativistic prejudices, to analyse systematically the peculiarities which distinguish research in the social sciences from that in other sciences. One very important question in this connexion is, how far these have an independent method of their own, and how far they are dependent on the methods of other sciences, such as physics, biology, psychology, and ethics.

Historically, the most important of these questions has probably been that of the relation between sociology and physics. And this is not to be wondered at if we recollect the extraordinary success of physics, especially in its technical applications, since the days of Galileo and Newton, and above all in the nineteenth century. The sociological investigator, who sees this success before him, is apt to draw this inference: if only we can apply the method of physics to the social sciences this must be the correct method or at any rate the

more fruitful one. And according to whether he considers this possible or not, he falls into a downright naturalism or into a no less downright antinaturalism. In the one case, he will urge that the methods of the so-called social sciences can only be termed scientific in so far as they are carried out according to the methods of the abstract natural sciences; in the second case, he will declare that these methods are in principle inapplicable to the central problems of the social sciences, since the latter are not concerned with spatio-temporal reality but with mental reality, which is of quite another kind. Each of these contending views has acquired, by appealing to speculative prejudices, a spurious air of philosophical solidity.

This is especially so with regard to the theory of mathematical knowledge and of the laws of nature, as we have briefly indicated. But as soon as these prejudices have been eradicated, the antithesis "naturalism—antinaturalism" is seen to be no less inadequate from a methodological point of view than the antithesis "dogmatism—relativism" which we have just reviewed. Thus the problem is revealed of carefully determining to what extent the two sciences actually use the same methods—for the reason that they both deal with empirical reality—and to what extent they diverge.

To bar the way to such an investigation by declaring that the laws of nature and the social laws are *toto coelo* different is as out of place as would be the statement that blue and yellow are *toto coelo* different: surely a comparison of blue and yellow would have to proceed from the assertion that both are colours. In this connexion, special importance attaches to comparative investigations into the function of natural laws in helping us to predict physical events and the function which social laws may be expected to fulfil—bearing in mind the present state of our knowledge of social laws—in the prediction of social phenomena. In considering the methodological problems of the social sciences we are rather inclined to underestimate the predictability of social events, although

in ordinary social intercourse we continually and successfully direct our actions in accordance with anticipations about the behaviour of other people—who are, moreover, often quite unknown to us. One need only recall the number of social prognoses involved in the sending off of a letter, or the buying of a railway ticket. The question of the comparative fruitfulness of natural and of social laws cannot anyway be settled by the airy remark that the one group is perfect, the other hopeless. I cannot deal here with these problems in detail, but only touch upon one point.

§ 3. It treats of the fact that the social sciences, in contrast to physics, lack universal laws and a uniform basis of reference for interpretations, which would constitute first principles in a hierarchy of laws governing the whole social sphere. It is therefore necessary to apply different methods to different problems; and the selection of the problems to be solved—itself influenced by practical considerations—comes to play a far greater rôle in directing research in the social sciences than in the natural sciences. No doubt the technician, too, has in a narrower sense his specific aims, which accordingly determine the amount of attention he gives to one special line of investigation, say the determination of material constants. But his mode of research is nevertheless determined in its basic features by general laws of nature, and his results find a prescribed place inside the framework of the whole system of natural science. On the other hand, in the social sciences, where general principles and a uniform basis of reference are not to the same degree at our disposal, we can only hope to find partial dominants, to discover firm islands of interconnection in the sea of social events. Hence methodological problems play a considerable part in the actual work of investigation. Here the investigator is faced, more often than in the natural sciences, with the task of reflecting what phenomena to correlate and what interdependences to bear in mind.

Precisely for this reason it is absolutely necessary to be clear

in one's mind about the aim of the enquiry, and to distinguish it clearly from the ulterior practical aims which have led to the establishment of the theoretical aim, and which are furthered by its attainment. If, for example, investigations into crowd psychology are instituted in order to draw from their results socio-political conclusions, the socio-psychological question itself can be sharply isolated from the measures which depend on the results of the question. The establishment of what I want to know fixes the subject of research, the (more or less provisional) determination of the routes by which I hope to gain this knowledge fixes the method of research, and both are independent of the reasons why I ascribe value to the knowledge. But it can by no means be inferred from the establishment of this logical independence that there exists also causal independence between the theoretical research work and these practical interests. For, as already pointed out, the motives of the choice of a problem are to a great extent rooted in practical interests, and the solution of the problem is intended as a basis for practical measures. It can, however, be claimed that once the problem has been set, the aim of enquiry is fixed also. The mode of research is then guided only by this aim, and not by the above-mentioned practical aims—even at the risk that the results finally obtained by the help of those methods will lead to practical measures which may appear undesirable to the investigator.

In view of the extremely complex nature of scientific investigation, which makes the answering of one question dependent on the answering of numerous preliminary and interrelated questions, even the posing of the problems becomes, to a certain extent, a purely scientific matter. It is a mistake, however, to speak in this case of " purposeless thinking," in order to contrast it with purposeful thinking guided by practical aims. In fact, the contrast is between thought *exclusively* guided by the aims of knowledge and thought *not exclusively* guided by them.

Only from this angle can the much-misunderstood meaning

of the *postulate* of *ethical neutrality* (*Wertfreiheit*), as Max Weber conceived it, be understood. It means simply the demand that research work should be purged of all those components which cannot be justified on scientific grounds but only in terms of non-scientific aims. This is the motto of truth for its own sake; and to live up to it the researcher must discipline himself to what Max Weber calls " intellectual integrity." Now if knowledge of absolute values or norms were possible, parallel to and independent of the knowledge of reality, it would not, according to the tendency of that postulate, be in any way ruled out as a subject for scientific research. For the postulate does not desire to draw a distinction between what is and what is not a legitimate object of knowledge, but to purge knowledge itself from extraneous elements.

But analysis shows that all concepts of value are relational concepts, such that every judgment of value presupposes a given reference-system of aims, and therefore a given axiological attitude. A judgment of value may be deducible from other judgments of value, but it cannot be expressed in its totality as an act of knowledge, i.e. as a judgment in the strict sense. There are two main reasons why this fact is so often overlooked. The first lies in the insufficient distinction drawn between value judgments for which objective validity is claimed and " judgments of taste." If someone declares on tasting some food that it is " good " (tasty), he only wants to affirm that he likes it at that moment; the idea that he will like the same food on another occasion is present, if at all, only implicitly. Therefore, a person making this judgment will generally consider the invitation to justify his judgment as out of place, and will answer it by saying, " I only mean that I like it at this moment." It is, however, quite otherwise with the judgment, " This man is good." For here, as a rule, a claim to " objectively correct judgment " is implied. Only propositions of the latter kind are problems for the philosophy of values, for those of the first group are obviously nothing else but assertions about the facts of personal preference,

bound up perhaps with anticipations of similar future facts. The confusion of these two types of propositions, however, leads to the fallacy of supposing that propositions of the second type, too, are assertions which can be understood in their totality without reference to a given system of ends.

The second consideration which was the main obstacle to the clear understanding of the meaning of axiological judgments is the fact that for the normal person a large number of aims are taken for granted. His action is continually being guided by them without their having attention focused on them. Thus any mode of action which furthers these aims comes to be regarded as " right " without qualification, since the ends for the realization of which it is the right means are no longer consciously considered. Only when a conflict of aims arises, making the person reflect on the maxims of his own conduct, does he become aware of the relativity of values to ends.

It will be clear, from what we have said, that the postulate of ethical neutrality does not in any way imply that sociology should neglect to study the axiological judgments passed on social life. On the contrary, these valuations must constitute one of the most important objects of the investigation; but they will be investigated all the more fruitfully if it is clearly grasped that all values are only relative to given ends, and that the ends implicitly presupposed by them must be made explicit.

§ 4. All this would be fairly obvious if counsels had not been darkened by speculative prejudices. Historically regarded, these can be traced back to Plato's exaggeration of the dualism between reality and idea, which reappears in modern idealistic philosophy as an exaggeration of the dualism between reality and validity, between truths of fact and truths of reason. Although much valuable thought of permanent significance is contained in the teaching of Plato and the great idealistic philosophers of modern times—especially Leibniz and Kant—nevertheless it can to-day be said that this dual-

ism, which is closely linked up with misconceptions as to the character of abstraction, the nature of concepts, and the content of mathematical (especially geometrical) thought, cannot be maintained. In view of this, however, the philosophical argument for an autonomous rational or irrational knowledge of values, which is based on the assumption of an independent realm of values, falls to the ground. The same prejudices are responsible also for misinterpretations of the concepts " meaning," " interpretation of meaning," and "understanding," which are fundamental to the social sciences. For the " meaning " of propositions is also conceived in terms of knowledge independent of reality, and accordingly it incorrectly appears as though the interpretation of meaning were entirely different from causal interpretation. In fact, however, the meaning of propositions or of other " products of objective mind " is nothing more than the content of those mental processes which find *expression* in them. Thus language is a system of symbols, which are symptoms of thoughts. Symptomatic relations are, however, only the reflection of real relations. For the fact S_1 is called a symptom of the fact S_2, if conclusions can be drawn from the presence of S_1 as to the past, present, and future existence of S_2. But that such conclusions follow is only because there exists between S_1 and S_2 a real relation, an empirical connexion. The real relation need not indeed be such that the symptom of one circumstance necessarily coincides with one of its causes. On the contrary, the symptom may equally well be an effect of the fact in question, or one of its regular concomitants due to the same cause as itself; and even if the symptom is one of its causes, a specially reliable symptom need not necessarily be a specially important cause. Hence the distinction between real ground (cause) and epistemological ground (symptom) which had only too often been confused with each other in pre-scientific, scientific, and philosophical thought was an important critical achievement.

But this distinction does not rely on the assumption that the

epistemological grounds belong to a different ontological sphere (that of so-called ideal objects) from the real grounds. This fallacy is primarily responsible for the thesis that the social sciences, *qua* sciences of interpretation as opposed to mere explanation, are non-empirical sciences dealing with an objective mind—that, for example, jurisprudence is independent of empirical psychology. Only when this misconception has been cleared up can the methodological problem of how closely psychology and the social sciences are related be clearly formulated. It must, indeed, be stressed that this way of stating the problem implies that there is a single psychological method with which the method (or methods) of the social sciences can be confronted. Now this might have been true—with a good many reservations—at the end of last century, when association psychology held the field. But to-day, a psychologist, asked whether according to his conception the methods of the social sciences coincide with those of psychology, would first have to make sure which psychological method was meant: whether that of introspective psychology, structure (*Gestalt*) psychology, psychoanalysis, characterology, behaviourism, or psychological anthropology. It would have to be made clear whether the descriptive methods (or, e.g. the explanatory methods) of psychology and of social science were to be compared and whether the questioner regarded self-observation or observation of others, or a combination of both processes, as constituting the essence of psychology.

After this explanation, it will readily be seen that the controversy about " psychologism " in the social sciences really boils down to the question whether the interpretation of social facts requires the help of a specific source of knowledge, or whether it is derived exclusively from internal or external experience. We realize that the latter is the case as soon as we grasp that the knowledge of social facts consists in the interpretation of the meaning of other people's behaviour, or of the phenomena associated with such behaviour (especially

the products, in the broadest sense, of human activities). For the phenomena of meaning do not, as already mentioned, constitute an autonomous sphere of knowledge. The interpretation of meaning is always a synthesis of internal and external experience, and accordingly, concepts of social facts must be constructed out of concepts of physical and of psychophysical facts. But social facts do not consist either of physical facts alone or of psychical (in the sense of purely intra-mental) facts alone.

Thus both extreme behaviourism and extreme psychologism must be rejected. If, however, psychology is defined so as to include the theory of human behaviour, the question whether the social sciences can be included under psychology must not be too hastily answered in the negative. It will have to be carefully shown how far the implicit aims of knowledge and the methods of enquiry are the same in both cases. It will then be seen that in these social sciences the arrangement of the data of observation and the formation of concepts proceed in accordance with a point of view different from that in the psychology or psychologies of human behaviour. At all events, there are gradual transitions between the different branches of psychology and the different social sciences. It must never be forgotten that the traditional boundaries between the sciences are bound up with definite stages of development in scientific research, and in any event remain extremely vague. The task of methodology is to isolate the various aspects under which these demarcations are brought about, and to analyse them in detail. Assuming we are agreed that the meaning, e.g. of human speech, possesses no transpersonal existence, but is merely a symptom of mental acts (or their contents) or of the mental attitude of individual persons, then the task presents itself of analysing these symptoms. The interpretation of meaning consists in making explicit all those implicit presuppositions which escape recognition if we start from the misconception that meaning is an inherent quality of ideal entities. Furthermore, we

have to be clear how far the interpretation should extend. We should like to show by an example of interpretation that often a consecutive series of explanations can be distinguished.

On the 6th of August, 1914, the commander of a British ship noticed certain movements of a coloured object, and assumed that they were to be explained as flag-signals, optical morse code signs. (1) He deciphers the words, "*Neptune*, etc., etc., 4 inches water in the signal room." That is the meaning of the communication. (2) He then grasps the purpose of the communication, which is to call upon his ship to give help. (3) He explains the *Neptune*'s S.O.S. as the result of a collision with an iceberg which he, the captain, had already been signalled as being in that region. (4) Finally, he concludes from the call for help that the *Neptune*, which he recognizes as a German ship, has not heard of the outbreak of war, since an enemy ship does not ordinarily appeal for help. Such a sharp division between the stages of interpretation is only rarely required for purposes of practical life, but it is all the more important for social science. If this aspect is not emphasized, the questions of the relation between objective and subjective interpretation of knowledge (e.g. in jurisprudence) or between the sociology of knowledge and the theory of knowledge cannot be treated with full justice to the facts.

§ 5. Attention must be paid to the clear determination of the means of interpretation no less than to its aims. That is not difficult to see as soon as one recognizes that when interpreting a phenomenon one does not regard the latter in isolation, but as placed in a more or less definite framework of experience. We have only to bring to mind the many presuppositions which are involved in the interpretation of a communication of a definite content by another person. In classifying the means of interpretation the most fundamental distinction is that between the individual facts or data and the general empirical rules or frames of reference, in terms of which they are interpreted. It is especially

important to observe the typical variations which occur in the means of interpretation, when the relations between the interpreting person and the persons whose behaviour is being interpreted undergo a change. Dr. Schütz,[1] who has examined this problem with special care, distinguishes several senses of the expression " social environment." In the first sense environment refers to the person immediately present to the interpreter. In the second sense it refers to contemporaries who do not belong to the immediate spatial environment of the observer. And finally, it refers to persons belonging to the past who cannot therefore under any circumstances be interrogated. Accordingly, the demand for objectivity of interpretation cannot be understood to mean that there is required merely an objective meaning unequivocally given. We must indicate the invariants as against the variants—which depend on differences in the " position " of the observer. A formal analogy to this is provided by the attempt made by the theory of relativity in physics to disclose universal invariants in natural processes. This analogy is also valid from the point of view of the psychology of knowledge (genetically) in the sense that in each case the problem has arisen out of the critical analysis of the relation between subject and object. Dr. Schütz has undertaken the very interesting attempt of considering the method of ideal types in the social sciences from the aspect of the postulate of invariants,[2] but we cannot in our limited space enter more closely into this question.

Nor can we enter into the problems of the " historical " element in the social sciences. We can only say, that e.g. the sharp contrast between the analytical and the historical school in economics disappears as soon as a number of mistakes concerning the relation between deductive and inductive methods are eliminated. The controversy then shows itself to be merely a difference of opinion as to the level of

[1] Cf. *Der sinnhafte Aufbau der sozialen Welt*, 1932, pp. 156 *seq.*

[2] Cf. *op. cit.* pp. 202 *seq.*

abstraction to which economic investigations should be pushed, as to the degree of universality of the laws which have to be set up, and as to the manner and degree in which they are to be checked by the facts. These important questions, however, cannot be decided *a priori* or *en bloc* but only by detailed analysis of the special peculiarities of the various classes of sociological problems.

§ 6. And now a word about the relevance of our critical analysis for the structure of sociological concepts. The concepts of social collectivities, like society, state and economy, can acquire a precise meaning only in terms of specific relations between individual persons. This principle was clearly understood by Max Weber, who applied it over a great part of the sociological field. Nevertheless, there is still plenty of scope for valuable detailed work on the same lines.

A closely connected problem is illustrated by the recent controversy between " Universalism " and " Individualism," which concerns the priority or " primacy " of Society over the Individual or *vice versa*. Unfortunately the concept of priority is used equivocally, and in the short space at our disposal we should like to indicate the various relevant meanings of this term.

First we must discuss the question of logico-ontological priority. This refers, on the one hand, to comparative degrees of complexity, the simpler object being regarded as logically prior to the more complex object of which it is a part. It refers, on the other hand, to the degree of independence of an object from other objects, the more independent object being regarded as prior to the less independent one. If priority is asserted in the first sense, then it is necessarily denied in the second sense—a point which is often overlooked.

Priority in these senses is often confounded with genetic, methodological or axiological priority. The problem of genetical priority plays to-day no longer an important

part in scientific investigation. It is recognized that from it no consequences about axiological priority can be deduced.

The question of methodological priority turns upon whether in social investigation we ought to begin with (supposedly unanalysable) social wholes or with the behaviour of individuals. In fact, however, as we have shown, even statements about social wholes (social collectivities) are nothing more than statements about the behaviour of more or less anonymous individuals. Deeper analysis shows that behind the apparently rigid antithesis are concealed certain divergences of method, e.g. the " macroscopical method " in the social sciences (Simmel) and the study of the influence of " *élites* " upon social formations. These divergences are not, however, irreconcilable.

Lastly, as regards the question of axiological priority between society and individual, it is clear, from our general remarks on the concept of value, that the question acquires a precise meaning only when it is related to a given hierarchy of ends. It is then seen that certain ethico-political postulates lie behind the thesis of the axiological primacy of society as against the individual; e.g. the postulate of ranking military strength above economic welfare, and similarly the postulate of assessing the worth of the individual according to his services in furthering this end. It is, therefore, no longer a question of theoretical insight, but one of practical decision, which cannot under any circumstances be deduced from theory.

§ 7. From the examples just given of the consequences for methodology which result from speculative prejudices, it should be evident that their logical exposure removes a serious obstacle to scientific progress. The next task is to draw up a systematic survey of the real methodological divergences which emerge. This will be best achieved by drawing up a scheme of possible methods of procedure according to which different actual methods can be com-

pared. I have attempted to do this in my book, and I have tried to illustrate its use by a series of examples. The elaboration of particular schemata adapted to certain groups of problems remains to be carried out in subsequent works. Here we must limit ourselves to a few explanatory words. Every scientific process may be divided into three phases: the setting of the problem, its working out, and its solution; but this does not mean that the phases are logically independent of one another.

The following questions then arise:

1. What knowledge is assumed in setting the problem?

2. What kind of additional knowledge is relevant to the solution of the problem?

3. What kind of knowledge is (*a*) presupposed, (*b*) acquired during the working out of the problem?

Further subdivisions are arrived at by classifying knowledge according to its chief types. Here we must distinguish: (*a*) knowledge of facts, (*b*) *a priori* knowledge, (*c*) knowledge of laws.

Moreover, knowledge may be of different degrees of clearness and distinctness, and its component parts can be ranged in order of function, significance and relevance for the enquiry in hand.[1]

This formal scheme provides the framework for an unlimited number of types of procedure, which may be ranged in order of kinship. As soon as such guiding lines are available for the various fields of sociological research, methodological controversies will for the most part be easily settled —in so far as it is possible to settle them at all in the given state of knowledge. The latter is not the case when, to settle the controversy, experience is assumed which cannot as yet be obtained.

First of all, then, the actual points of divergence between contending methods must be ascertained, and then the

[1] *Anordnung, Deregierungsordnung* (*Rangordnung*) and *Auswahlordnung*. Cf. *Methodenlehre des Sozialwissenschaften*, pp. 124–5.

protagonist of any particular method will have to explain why he considers his method preferable to the others. By this means, research workers will be compelled to take into account to a greater degree than in the past whether they have been guided in their choice of one method and rejection of another exclusively by scientific considerations. Intellectual integrity will thus be encouraged. This process of clarification, however, according to all anticipations, will have more far-reaching consequences for the progress of sociological knowledge. By bringing together criteria of proof and disproof it will at the same time provide a basis for co-ordinating different lines of enquiry. This will enable particularly statistical research to be carried on much more systematically than has been the case hitherto.

To summarize: the time is now ripe for a radical clarification of methodological questions in the social sciences, which may be expected to react very favourably on the future development of sociological knowledge. But it must finally be stressed that this requires the organized collaboration of a great number of investigators.

THE DEVELOPMENT OF LANDOWNERSHIP, 1873–1925, WITH SPECIAL REFERENCE TO BEDFORDSHIRE

By HENRY DURANT

IN the House of Commons, November 17, 1920, Lord Halifax (then Hon. E. F. L. Wood) spoke of the "revolution in the system of land tenure in England which is going on, of course, before everyone's eyes at present." And again, on December 9, 1924, the same speaker said: "... Taking a view of British agriculture that is not confined to this year or to next year, there is something like a silent revolution in progress within its borders. We are, unless I mistake, witnessing in England the gradual disappearance of the old landowning class."

It is much to be regretted that no systematic survey of the ownership of land in this country has been undertaken since the celebrated *New Domesday Book* of 1873. It is only by such means that the question which the then Minister of Agriculture raised can be answered. Much valuable information could also be obtained from the statistics of land sales in each year; again it is to be regretted that no official steps are taken to record these details. The available official statistics (see below) are extraordinarily meagre, and because of this it has been thought worth while to publish the accompanying figures.[1]

An investigation into the County of Bedfordshire has been made and the position revealed is contrasted with that shown in the Government Return of 1873 for that county. The figures refer to properties, not holdings, and only estates over 100 acres have been included.

[1] For a discussion of the existing statistics and their value see Stamp, *British Incomes and Property*, 1916, pp. 71–80.

BEDFORDSHIRE: PRIVATE ESTATES OF OVER 100 ACRES

1925 COMPARED WITH 1873 [1]

Area of Estates.	1873. No. of Estates.	1873. Total Area.	1925. No. of Estates.	1925. Total Area.	Increase +. Decrease −. Estates.	Increase +. Decrease −. Area.
acres.		acres.		acres.		acres.
100–200 .	138	19,503	176	24,942	+ 38	+ 5,439
201–300 .	69	17,010	90	22,063	+ 21	+ 5,053
301–400 .	30	10,263	54	18,705	+ 24	+ 8,442
401–500 .	18	8,122	23	10,447	+ 5	+ 2,325
501–600 .	5	2,757	20	10,930	+ 15	+ 8,173
601–700 .	9	5,748	10	6,476	+ 1	+ 728
701–800 .	10	7,494	7	5,230	− 3	− 2,264
801–900 .	8	6,856	5	4,273	− 3	− 2,583
901–1,000 .	6	5,701	1	954	− 5	− 4,747
over 1,000 .	45	158,151	27	99,996	− 18	− 58,155
Total .	338	241,605	413	204,016	+ 75	− 37,589

An examination of this table reveals two outstanding facts. There has been an increase in the total area covered and in the number of owners in each of the categories 100–200 acres, 200–300 acres, up to 600–700 acres. Secondly, a decline is

[1] Many questions have been raised concerning the reliability of the statistics for 1873; see, e.g., Brodrick, *English Land and English Landlords*, 1881, Chap. III. In the main the criticisms have been that the figures do not disclose the true preponderance of the large landowners; thus clergymen have been included as proprietors of the glebe land, whereas in many cases it in fact belonged to a person already included as the owner of large landed estates. The figures for 1925 have been obtained by an examination of the Bedfordshire rate-books. These give, amongst other details, the name and address of the occupier, the name of the proprietor, and the area of the property concerned. The rate-books are compiled on the basis of parishes, and it will be observed that a difficulty arises when the parish boundaries cut a property of more than 100 acres into pieces of less than this area; the separate entries in the rate-books could then easily be overlooked. These omissions were guarded against by extracting those properties under 100 acres which were noted as being "part of." If the total did not eventually reach 100 acres, they were then excluded. Further check was forthcoming from *Kelly's Directory*, which mentioned the principal landowners in each parish and also indicated those farmers whose farms exceeded 150 acres in extent (of course it does not follow that such farmers own their farms).

It is of interest to record that information concerning one very large estate was obtained from independent sources. The figures thus secured, when compared with those extracted from the rate-books, showed a divergence of not more than 3–4 per cent.

My thanks must be extended to all those officials, particularly Mr. F. G. Emmison, Clerk of the Records, who have so kindly rendered valuable assistance.

recorded in each of the other categories, both for the number of owners and for the total area covered. In other words, the number of smaller properties up to 700 acres in area has increased. Properties of a greater area than this have declined in number and extent.

It will also be seen that the total area covered by private estates over 100 acres in area has declined. It may be assumed, therefore, that the total area covered by properties of less than 100 acres has increased, and that presumably the number of the owners concerned has also increased. The present conclusion is subject to one qualification. The table excludes properties of public bodies (except those of the Bedfordshire County Council), and the estates in public ownership have almost certainly grown since 1873. This increase, therefore, would express itself in a growth of the area excluded by the table.

These conclusions seem to indicate such a clear trend during the period under review that it is of interest to discuss the forces that have been at work. Although they are divided under headings, this is merely for the purpose of exposition. In the actual development the various factors have been closely intertwined and mutually reacting, in some cases stimulating, in others counteracting each other. The order in which they are set out does not indicate any attempted estimate of their relative importance.

Public Ownership of Land

The statistics deal only with properties in private possession. Accordingly, if land has passed into the hands of public bodies since 1873, it would be included in the first set of statistics, but excluded from the second. During the last sixty years we have witnessed a growth in public ownership; land has been acquired by local authorities for many purposes which are too well known to need enumeration. Perhaps among public authorities special mention should be made of the Forestry Commissioners since they now own such vast

tracts of land. Their importance in the present instance, however, is slight, since in 1925 they owned, in Bedfordshire, less than 400 acres.

If the increase in public ownership be accepted as part explanation of the greater area excluded by the statistics of 1925 compared with those of 1873, the question of how this factor has operated over the different categories is still open. Has its effect been equally spread over all the estates? In other words, have the owners of small estates disposed of their properties to the public authorities equally with the owners of large estates? There seems no inherent reason why this should not be the case, but the evidence suggests that the effect has been greatest on the large estates. The Bedfordshire records indicated such a trend, and there are good reasons for accepting them as typical for the remainder of England.

Our conclusions are, therefore, that public ownership has increased, and that for its effects we must look mainly to the larger estates, in causing a decrease in their size.

The Declining Political Significance of Landownership

Writing in 1878, Caird [1] pointed out the importance of landownership in relation to the political influence wielded by different groups in society; at the same time he noted that this importance was declining. Since he wrote, political influence as a function of landownership has progressively decreased. Only ten years afterwards the famous County Council Act was passed which did so much to curtail the power of the landed gentry in local government. The successive steps whereby the electorate has been widened to coincide (with few exceptions) with all those over 21 years of age have been further factors in bringing about this decline.

[1] J. Caird, *The Landed Interest and the Supply of Food*, 1878, pp. 71–73, "Three-fourths of the land in England have long been held by a comparatively small body of great landowners. From the Revolution in 1688 till the Reform Bill, 1831, all political power was in their hands. . . . By those measures (of 1831 and 1867) the numbers and political strength of the tenant farmer class were largely increased."

It is not argued, of course, that the landowner's influence has disappeared. Anyone acquainted with local or even parliamentary elections will have ample evidence to the contrary. And it is necessary only to bear in mind the number of large landowners in the House of Lords to discredit such a view. Nor is it argued that this aspect of landownership has been consciously present to those buying or selling; it probably expresses itself in a refusal to be longer bothered with the possession of land. Ownership of land as compared with other forms of property has always involved its special responsibilities and the changed manner of living among the upper social classes has tended to make these duties more onerous and the benefits less attractive. At one time the needs of a leisured life could be met almost completely by the possession of an estate. Nowadays, a greater and greater concentration on London as the centre of Society, more travelling abroad with, probably, wintering in a foreign country, have all brought about a divorce of the landowner from his country seat. An estate now comes to be regarded solely as a burden and hence the willingness to dispose of it.

The Policy of the State as Expressed in Legislation

It will be convenient to divide the discussion into legislation concerning public finance and all other legislation that has affected landownership and land occupancy.

In public finance, the taxation of land values, whilst a matter for very acrimonious debate, has never played an important rôle. The machinery set up by Lloyd George for the purpose of land valuation remained in existence, however, till 1925, and as such it was a contingent threat to those who owned the land. For the important aspects of public finance it is to the Death Duties we must turn.

Commencing with Sir William Harcourt's famous budget of 1894, and with the Lloyd George Budget of 1909 as its high light, there has been a continuous and progressive policy to make estates liable to Death Duties. The scales of taxa-

tion have been increased rapidly, as is shown by the following figures for the Estate Duty.[1]

Estate Valued.	Estate Duty Payable.	
£	1894. %	1925. %
50,000	5	11
250,000	7	21
1,000,000 . . .	8	30
2,000,000 and over . .	8	40

The five fiscal years ending 1925 produced no less than £273,000,000 (approx.) from this source, the figures for 1925 being more than £59,000,000.[2] Undoubtedly, the effect of these taxes has been in some cases to cause landed estates to be sold in order to meet the payment of the Duties.

It is suggested, however, that the significance of this factor in breaking up landed estates can easily be overestimated. Thus one witness before the Haversham Committee,[3] who strongly emphasized the adverse effect of legislation, was unable, in answer to a question, to produce any one specific instance in support of his case. It is known that Death Duties will inevitably have to be paid, and hence they can be insured against, in which case the charge comes out of income and not capital. At the same time we must note that the Minority Report to that of the above Committee was based on the belief that adverse legislation was the only cause of the selling up of estates.

Legislation, other than Finance Acts, which has affected the ownership of land falls into two classes. During the second half of the last century, much was written on the legal hindrances to land transference[4] and they have occupied the

[1] Inland Revenue Report, 1926, Cmd. 2547. [2] *Ibid.*

[3] Departmental Committee on Tenant Farmers and Sales of Estates, 1912, Cmd. 6030, Minutes of Evidence, 2377.

[4] E.g. J. Caird, *op. cit.*, chap. XII. Wren Hoskyns, *The English Land System*, 1873.

attention of the legislature on many occasions. The most important measures, however, were the Acts of 1925 [1] whose effect has been felt during the years following the period that is under review. It is difficult, therefore, to discover any evidence that the attitude of landowners had been affected by the growing ease of land transference prior to 1925.

Secondly, there is the legislation which has defined or restricted the function of the landlord in relation to the tenant-farmer. The tradition in English agriculture has been for the landlord to supply capital to the farmer in the shape of, e.g., buildings. In addition the landlord exercised a function in controlling to some extent the manner in which the land was farmed. Even during the most prosperous period of British agriculture the question of landlord and tenant was continuously engaging public attention. One needs only to glance at the voluminous literature that was produced from about 1840 onwards in the Irish land controversy to be convinced of this fact. With the formation in 1879 of the Irish National Land League the problem came to a head, and the series of Acts passed by successive Governments testify to the gravity of the issue and its intractability. It is impossible to hold that legislation for Ireland did not have its reactions on the attitude adopted to the situation in England. Perhaps this connexion is of historical interest only, and it is necessary to examine the actual legislation which was passed in relation to England to discover the effect of the enactments. The process of the legislation can be summed up by saying that the landlord has been progressively stripped of his functions and deprived of existing rights against the farmer.

The Agricultural Holdings Act, 1908, gave the tenant two important rights. Any restriction on his freedom of cropping was now removed.[2] Secondly, unless the landlord could show " good or sufficient cause " for terminating or refusing to renew a lease the tenant must be compensated.[3] Thus

[1] Law of Property Acts, 15 Geo. V, Cap. 18–23.
[2] Sec. 26. [3] Sec. 11.

agricultural land was placed on a different footing from other lands, and the tenant acquired more than a mere right of leasing. The recognition of this principle was important, but in practice, largely owing to judicial interpretation, the clause did not meet the need it was designed to cover, and the Agriculture Act, 1920, remedied this shortcoming. Not only did it define more precisely when the tenant was entitled to compensation,[1] but it laid down the amount to be paid. The same Act gave the tenant the right to demand arbitration on the question of rent.[2] As the debates in the House show, the Government included with difficulty a clause [3] empowering the Minister of Agriculture to appoint to an estate a Receiver and Manager where it seemed necessary or desirable. It is true that the next Act to be passed dealing with agriculture [4] repealed this provision, but an indication had been given of the possible direction of development of legislation. Finally, the 1923 Act [5] gave the tenant farmer almost complete security of tenure.

It is clear, therefore, that the function and rights of the landlord have tended to be restricted for the benefit of the tenant farmer. This factor has undoubtedly been a contributing cause to the selling up of estates.

Foreign Competition in Agricultural Produce

From causes which are well known, English agriculture experienced a time of unprecedented and unrepeated prosperity during the two decades 1850–1870. For the next twenty or twenty-five years a depression of unparalleled severity and duration seized the land.

Arising out of the developments in transport and the increase in farming in the New World, foreign competition caused prices of agricultural produce in England to fall catastrophically. All classes, landowners, tenant-farmers, and labourers suffered. The latter adopted still more vigor-

[1] Sec. 10, sub-sec. 6. [2] Sec. 10, sub-sec. 3. [3] Sec. 4, sub-sec. 7.
[4] Corn Production Acts (Repeal) Act, 1921. [5] Agricultural Holdings Act, 1923.

ously the policy they were already following of migrating abroad or to the towns. As the result, we get the constant complaints of the lack of labour on the land and the poor quality of what remained.[1] The tenant-farmer had perforce to make the best of a very bad job, since he knew no other way to make a living. Faced in some cases with a deficit—on nine occasions during the thirty years ended 1895, the Bedford Estates showed a net deficit [2]—and in others with a very small return on the capital invested, landowners had no incentive to retain the land.

It is difficult to gauge how far these stresses and strains brought about immediate changes in ownership. Just as clearly as the decrease in the return on land supplied one reason for disposing of ownership, so it supplied another for not selling, since the price obtainable would be at its lowest. Moreover, there would be no incentive to purchase. Whilst it is certain that the decrease in rents which had followed the decrease in prices caused landownership to be a source of very meagre income, it must be borne in mind that the net return on Consols was little more than 2 per cent. at the end of the nineteenth century. From the evidence we have it is clear that many thousands of acres became derelict, and this is just the result we should anticipate rather than a tremendous change-over in ownership of land. In every country land changes hands rapidly and to a great extent during periods of prosperity and not during depression.[3]

If this position be accepted, then it is probably correct to say that the effect on ownership of the depression was not felt till the years just prior to the War. As prices slowly recovered so owners, remembering their experiences, took the opportunity to sell. Jesse Collings notes that in one week in June, 1910, 72,622 acres were offered for sale in 36 counties of England. In the corresponding week in 1911, 98,300

[1] See, e.g., Rider Haggard's *Rural England*, 1901.
[2] Duke of Bedford, *A Great Agricultural Estate*, 1896.
[3] Henry George, *Progress and Poverty* (1878).

acres were offered, and in 1912 one firm alone during four months " put through the market land to the value of no less than 1¼ millions Sterling." [1] These figures are supported by T. E. Marks,[2] who quotes the following statistics :

ACREAGE OF AGRICULTURAL LAND IN ENGLAND AND WALES, REGISTERED AT THE ESTATE EXCHANGE AS SOLD

Year	Acreage
1908	97,263
1909	100,273
1910	114,661
1911	188,009
1912	192,624

If any further evidence be needed we find it in the Report of the Haversham Committee, Sec. 6, " The Committee are satisfied that there are an abnormal number of estates being broken up and sold at the present time " (1912).

It must be remembered that agricultural rents are comparatively inelastic, and it is difficult, if not impossible, for the landlord to take full advantage of any economic improvement. Selling the land on a rising market and investing the money elsewhere offers accordingly considerable attractions, and it is noteworthy that in Bedfordshire of the large estates, those have best maintained their size which do not form the owner's principal source of income. In other words, when landownership has been allied with trade or industry it has been regarded as a satisfactory alternative or additional investment. This conclusion is confirmed by an enquiry into the continuance of landowning round Hertford.[3]

Rising Prices of Agricultural Land after the War

We have previously remarked that it is in times of prosperity that land changes hands to a considerable extent, and this seems to have been the case in the years following the War.

The official statistics of ownership are very slight, but it is

[1] *Colonization of Rural England*, Vol. I, 1914. 259.
[2] *The Land and the Commonwealth*, 1913, pp. 30–31.
[3] By Roper Power (unpublished).

interesting to examine the only figures which have been published during the last sixty years.[1]

PERCENTAGE OF ACREAGE OWNED BY OCCUPIERS

1920	15·5
1924	25·4

PERCENTAGE OF OCCUPIERS OWNING HOLDINGS

1920	13·7
1924	23·0

The official comment may be quoted: ". . . Between 1919 and 1921 the numbers showed a material increase, due no doubt to farms and holdings being bought for occupation." There are two things to note here. The tremendous shift of ownership that occurred during these years. Secondly, the evidence that, of the purchasers, the tenants formed a considerable body.

The quotations at the beginning of this article indicated the enormous turnover in land from 1919 onwards. The figures which we have been able to collect for Bedfordshire demonstrate such a process. Parish records, in the shape of Supplementary Valuation Lists, form a complete record of the changes in the size of properties. Unfortunately, these lists were available in the case of only one Union out of five, yet even these gave clear indication of the selling of estates that occurred. We found in this Union ten large estates that were sold and broken up during the years 1919 and 1921.

There is undoubtedly a tendency, when a suitable opportunity presents itself, to transform wealth in land into a more mobile form. This was noted in the evidence before the Haversham Committee, as was also the tendency to sell land in England and to purchase land abroad. Both these processes were at work after the War, and specific instances could be quoted in support. It is interesting to note that the reverse tendency is also present. One may hazard a guess that the Joint Stock Banks are incomparably the largest holders of real estate in Great Britain.

[1] *Agricultural Output*, 1925, Cmd. 2815, p. 13.

The growth in the number of owner-occupiers is the second fact brought out by the official statistics. It is also indicated by Marks,[1] by Orwin and Peel,[2] and by the Bedfordshire records which were consulted. (Rate books are compiled on the basis of parishes, and it is impossible to ascertain from them the actual percentage of owner-occupiers.)

We are, however, faced with the problem that all the evidence available points to the reluctance of tenants to purchase. The authors just quoted emphasize this point, and the Report of the Haversham Committee also gives abundant testimony : Sec. 76 : " The evidence we have heard makes it quite clear that tenants do not desire to purchase their farms except as an alternative to leaving altogether." Sec. 77 : " Of the farmers who gave evidence before us only three advocated purchase save as an absolute necessity, while other witnesses were strongly antagonistic to the proposal in the interest of the farmers themselves. One land agent and surveyor stated that, putting himself in a tenant's position, nothing would induce him to buy."

It is clear why the desire to purchase is absent. As a tenant his capital is sufficient, but if the farmer has to purchase the land as well he experiences at once a shortage of capital. " I believe that the majority of tenant farmers in England and Wales are not in a position to pay down one-fifth of the purchase money, and in my opinion, such a scheme would result in the starving of the farm owing to the lack of working capital." [3]

This " stretching of one capital to do the work of two " adversely affects farming, and adversely affects the financial position of the farmer himself; yet as we have seen tenants have bought their holdings.

We have to conclude that they have been obliged to purchase, thereby worsening their position. We have remarked that agricultural rents are comparatively inelastic

[1] *Op. cit.*, p. 31. [2] *The Tenure of Agricultural Land*, 1925, p. 18.
[3] James Parker, M.P., *Reservation to the Report of Haversham Committee.*

—the landlord has undoubtedly acted as a breaker between the tenant and the economic storms. This inelastic rent has gone and its place has been taken by a much more elastic bank charge.

Summary

It seems that most of the forces we have discussed have been working in one direction : to cause the selling up of the landed estates and the disappearance to a considerable extent of the landowning class.[1] Since all the factors have been present it is useless to discuss whether the same development would have occurred if any one had been absent. We can note that legislation and taxation have rendered landownership less attractive, but it must also be observed that the selling of land on a large scale already took place before e.g. the Budget of 1909 and the subsequent campaign against landlordism. The evidence available suggests that the disadvantages of large-scale ownership is a contributory motive to selling, but the decisive condition is the opportunity to realise a favourable price. It is only on this basis that we can attempt to explain the enormous transfer which took place in 1919 and the following years.

The Future Trend of Ownership of Rural Land

In view of the propaganda on behalf of smallholdings (defined officially as holdings of less than 50 acres), it is important to discuss the prospects of such a development in English farming. We have witnessed continuous attempts

[1] Delisle Burns, in *Leisure in the Modern World*, 1932, Appendix, p. 210, gives a letter received from a local official of Gloucestershire who had lived in that county for more than seventy years: "Near the end of last century much of the land was farmed by families that had been continuously in possession for two centuries or more. These have nearly all gone. . . . One family has vanished after a stay of six centuries. Most of their successors are descendants from workers. . . .

"A significant feature is the appearance and total disappearance in little more than a century of the Wesleyan and Methodist farmer. It seems analogous to the case of the Quakers who came and went just previously."

The latter suggestion opens a fascinating field of study which, however, cannot be developed here.

to initiate smallholdings in this country on a large scale, either by letting to the occupiers or by giving assistance to purchasers. Yet writing in 1925 Orwin and Peel noted that the number of smallholdings had decreased over the previous fifteen years.[1] They say: ". . . It [small holding] entails a life of greater toil in exchange for an incommensurate reward. . . . It cannot be argued that such a condition is capable of reproduction in this country where the comparison between the material wealth of rural and urban workers is so much more easily made." In May of last year the Government were petitioned by smallholders in Scotland that they should be taken off the land.[2]

Granted the present economic and social complexion of England it can be said that smallholding as a really widespread movement cannot take roots. In those countries where it has been successful we find that a large proportion of the country is organised on that basis: the Government lends them a very sympathetic ear, all the necessary commercial and financial facilities are forthcoming. In no country has this organization been built up by the smallholders themselves. They are not in a position to do it. Until and unless we witness the growth of these ancillary services, the mere provision of smallholdings will not be sufficient to plant a new and important social group in England.[3]

In a recent paper [4] it was suggested that a probable develop-

[1] *Op. cit.*, p. 12.

[2] 304 H.C. Deb. 5 S, 1728 and 1731.

[3] These conclusions are confirmed in the most recent book on agriculture, *The Agricultural Dilemma*, a report of an enquiry organized by Viscount Astor and Mr. B. Seebohm Rowntree, 1935, p. 75, "But a far grosser disregard of the interest of smallholders would be entailed by an attempt, in the face of the present trends of markets, to effect a large multiplication of their [smallholders] numbers."

See also the report by A. W. Menzies-Kitchen for the Carnegie Trustees, January 1936. As with the investigators mentioned above, Menzies-Kitchen "started with every wish to report favourably" but the facts forced him to arrive at the same conclusion, viz. the smallholding has little chance of success except as a unit in a co-operative group system.

[4] E. P. Weller, *The Future of the Ownership of Rural Land in England*, 1935—The Chartered Surveyors' Institution.

ment in British agriculture was the growth of the Joint Stock Company. There are several reasons for accepting such a view, among which the following are outstanding:

(*a*) It seems that in the existing conditions the provision of capital in farming is becoming more difficult as the result of the disappearance of the wealthy landowner. The owner-farmer often has to resort to mortgages or loans on his stock to enable him to continue.

(*b*) There is an increasing difficulty in finding remunerative outlets for the investment of capital. It is a commonplace of our time that there are enormous liquid funds seeking investment.

(*c*) With Tariffs, quotas and Marketing Boards and all the paraphernalia of latter-day protection we can, perhaps, anticipate an era of stable prices in agriculture. Thus a very considerable speculative element will be removed and, moreover, the tremendous growth in dairy farming and cattle raising is another feature which makes for stability of results.

In market gardening, particularly, we have already witnessed a stretching back of the large wholesalers to become growers themselves. In the Midlands there are Limited Liability Companies operating in agriculture. It is probable that we shall see these movements developing and that the figure of the landlord will be supplanted by the anonymous figure of Capital.[1]

[1] See also the conclusion of Astor and Rowntree, *op. cit.*, Chap. V: " . . . The larger farms are becoming more formidable competitors to the family farms (defined broadly as between 50–150 acres), despite the rigidity of labour costs."

For a discussion on the industrialization of British agriculture and the attendant advantages, see Leake, *Unity, National and Imperial*, 1935.

BOOK REVIEWS

THE MEASUREMENT OF POPULATION GROWTH. By Robert R. Kuczynski. *Sidgwick & Jackson*, 1935. 12*s*. 6*d*. net.

If a social statistician were asked to say what were the most fundamental problems within his rather wide field of interests, he would not be far wrong if he replied: "The measurement of the growth and character of populations, and an analysis of the forces which determine their variation." The amount of land—and the natural wealth associated with it at any given time—is strictly limited, but human beings continue to multiply. Expanding nations, finding an insufficient outlet for their adequate industrial development, are tempted to make war upon peaceful neighbours. Wars can only be averted when an effective court of international justice is established, whereby the economic exploitation of the different parts of the world's surface is shared out upon an equitable basis. And even then the state of our civilization will be determined by the quality of the people who constitute the various nations. Accordingly, it goes without saying that this book, dealing as it does with the first in logical order of the problems defined above, namely, the measurement of population growth, and written by one of the greatest living authorities on the subject, is a work of outstanding importance.

Those who are acquainted with the earlier writings of Dr. Kuczynski, and who know how much he has contributed to a right understanding of the questions at issue in the examination of factors which influence the trend of population, will be grateful for the lucid and detailed description here given of the methods regarded by the author as best calculated to achieve their various purposes, including those he has himself originated which are among the most illuminating in their particular class. But the book is not confined to a study of statistical method. Comparative tables, invaluable for reference, are given, illustrating the population position throughout Europe, in Australia and New Zealand, and, in some cases, the United States and Canada.

Attention is directed on the first page of the Introduction to a definition of terms which have proved a stumbling-block to many owing to their use in ambiguous senses. Fertility, the actual production of children, is distinguished from fecundity, the production which "would be reached if all females gave birth to as many children as they possibly could." Clearly it should not prove impossible to measure fertility, and this book is devoted primarily to a critical discussion of the procedure followed in such measurements. But, in the nature of things, no data exist for the determination of variations in fecundity. It is, therefore, a little surprising to read on

page 3 that, until the advent of birth-control, the ratio of fertility to fecundity among married women may be assumed to have remained on the whole fairly constant, and that " fecundity itself, of course, changed—but such changes were seldom permanent, and, if permanent, were seldom considerable." After indicating, in a preliminary chapter, simple means for testing the accuracy of published statistics of births, the author proceeds to compare various attempts made to measure fertility (1) by the exclusive use of vital statistics—i.e. statistics derived from the registration of marriages and births; (2) by the exclusive use of census statistics; and (3) by the combined use of vital and census statistics, the ideal method. The last two chapters deal with the measurement of mortality and the balance of births and deaths, the crucial problem being to ascertain the net reproduction rate of each country under consideration. Are fertility and mortality such that 1,000 newly born girls will in the course of their lives produce at least as many girls to replace themselves? If not, the population is in the long run bound to decline.

Dr. Kuczynski is not sparing in his criticism of the statistical devices adopted for the measurement of changes in population by other, official and lay, authorities—or those who have been accepted as such—and, in the opinion of the reviewer, much of his criticism is well founded. Readers run the risk, indeed, of being left with the bewildering impression that quite a substantial part of the conclusions on these matters derived in the past, even from the statistics of Somerset House, are inaccurate and misleading. That would be unfortunate. Nevertheless, one may venture to hope that the sting of the author's pen will induce all who are attacked to re-examine and, where necessary, amend their ways. Haply some may venture the defence that the object they had in view differed from that which interested their critic and consequently it has been misconceived by him.

But sins of omission may be as serious as sins of commission, and no defence is possible of so glaring and persistent an omission as the age of the mother in the English Register of Births, seeing that it severely limits the accurate assessment of our fertility as a nation. Time and again in their annual reports, the Registrars-General have themselves commented upon and regretted this lack of essential data, but it still remains unremedied. As it is perhaps the weakest point in the vital statistics of this country, which is justly proud of its records in so many respects, it may not be unfitting to repeat one among several quotations on the matter which the author has selected from official sources. In the *Statistical Review of England and Wales*, for the year 1922, text (p. 137), we read that " the system of birth registration in this country, which has been retained in its present form from the date of its inception, nearly ninety years ago, is quite inadequate for the purpose (i.e. of measuring fertility); beyond the bare

identification of the mother in the birth record, no particulars are available as to her age, the date of her marriage, the number of existing children, and other information all of which is essential for an accurate and continuous examination of the changes in fertility from time to time."

Every Medical Officer of Health and every student of social science would do well to buy and to digest the contents of this valuable book.

D. CARADOG JONES.

SOCIETY AND LITERATURE

GERMANY IN THE EIGHTEENTH CENTURY: THE SOCIAL BACKGROUND OF THE LITERARY REVIVAL. By W. H. Bruford. *Cambridge University Press*, 1935. 15*s*.

THE TYRANNY OF GREECE OVER GERMANY. A Study of the Influence Exercised by Greek Art and Poetry over the Great German Writers of the Eighteenth to the Twentieth Centuries. By E. M. Butler. *Cambridge University Press*. 1935. 15*s*.

There are two ways of sociological approach to literature:

(1) A study of the general interaction of society on literature and of literature on society. Such a general sociology of literature has to analyse the relations between literature and social development. These relations include on the one hand the influence of society on literary production, on its forms, its content, its success, and its failure, and on the other hand the influence of literature on the moulding of the feelings and ideologies of the social animal called man.

(2) A study of the sociological interactions of a specific literature, an attempt to demonstrate the causes and effects of literature in a given society.

Whilst only a small beginning has been made in the general sociology of literature, an increase is to be noticed recently in studies of the second type. More and more students of Modern Languages no longer limit themselves to a purely philological or aesthetic interpretation of literature, but endeavour in a synthetic way to illuminate the correlations between social, political, and literary development.

Professor Bruford's book is a sound example of this tendency. Its main aim is " to provide material for a sociological study of eighteenth-century literature by describing the chief classes of society in that age, and the political and economic conditions under which they lived." This aim is admirably fulfilled. The picture which the author gives of the " Political Structure and System of Government " (Part I), of the " Old Order of Society—Nobility and Peasantry " (Part II), and of the " New Order of

Society—the Middle Class" (Part III), is based on a detailed and accurate knowledge and drawn with skill and an excellent sense of proportion. Compared with several German books on the subject, the clarity and realistic attitude of the author make his study indispensable to any student of German social and cultural development.

Take, for instance, the description of the middle class: firstly a retrospect of the history of German towns is given, then the guild system of industry and its growing inadequacy in the eighteenth century are indicated; then follows a survey of German trade and industry in the eighteenth century and their chief centres; then the municipal government and the structure of town society are discussed, the author duly stressing the different structure of society in the free or imperial towns from that in the territorial towns ("*Landstädte*") under the control of a territorial ruler. In comparison with the development in England, the enormous loss of independence and the political weakness of the German citizen is striking. The next two chapters deal mainly with the outlook and education of the older type of average burgher and of the newer type of professional man coming from this middle class.

It is interesting to observe how on the one hand this new professional class retained the typical virtues of the older middle class, its honesty, industry, and "sweet reasonableness," but on the other hand could not resist the contagion of aristocratic ways of living as a result of its contacts with the ruling class. The author should have stressed more this ambiguous attitude of the middle-class intelligentsia, which vacillated in the eighteenth, as well as in the nineteenth century, between a proud self-consciousness of its achievements and an imitation of the style of the aristocracy. Similarly, the important formation of a new *élite* of "*Bildung*," drawn from this professional class and from the enlightened aristocracy at small courts like Weimar or in the *Salons* of the Berlin Jewesses at the end of the century, is not analysed with sufficient penetration. Here the valuable sociological study of H. Weil, *Die Entstehung des deutschen Bildungsprincips* (Bonn, 1930) could have been helpful. (This book is not mentioned in the bibliography at the end.)

The fourth and last part of the book "Reactions on Literature," is chiefly interesting, if not completely satisfying, for the sociologist of literature. In its first chapter the status of the profession of letters in England and Germany is compared and the reasons for that of German writers being less favourable are examined. Hitherto, historians of German civilization have nearly always overlooked the fact that in the eighteenth century, Germany had developed both a classical literature by a few famous poets and thinkers and at the same time an intellectual proletariat.

Among the influences of political, economic, and social conditions on literature which are examined in the last chapter, two interactions are indicated:

(*a*) The effect of the political disintegration of Germany, which, in comparison with France or England, resulted in lack of a great national centre and therefore of national style and national subjects, as a consequence of which German literature became unsocial, individualistic, bookish, and open to foreign influences.

(*b*) The effect of the provincialism of the masses and the absence of possibilities for political power in the middle classes, which caused the escape of a cultural *élite* into a cosmopolitan world of its own, into the worship of Greece. This is a very fruitful point of view, which should perhaps be worked out in fuller detail.

The reader who turns to the very spirited and brilliantly written book by Miss Butler in the hope of such more elaborated explanation of the sociological significance of German Hellenism will be disappointed. For this book has no intention of illustrating the social background of the movement, but sets out " to measure the intensity and the nature of the tyranny of the Greeks over some great, outstanding individuals," such as Winkelmann, Goethe, Schiller, Hoelderlin, Heine, Nietzsche, George, etc.

The specific value resulting from this able, but somewhat individualistic approach to the history of an idea—stressing, as it does, Goethe's daimon, Nietzsche's Superman and George's Maximin as three mythical creations of the classical worship of Greece—cannot be dealt with in this journal. But the sociologist of literature must consider Miss Butler's efforts to understand German Hellenism from Winkelmann to George as an expression of a national character. This author works out a unifying construction of " national temperament " similar to that of Hippolyte Taine in his famous " *History of English Literature* ". According to her the Germans were and are always at the mercy of absolute ideas, the slaves of a " dangerous idealism." From that follows, negatively, " their unbalanced enthusiasm, their helpless subjection to catchwords, fashions, and aesthetic theories," but also, positively, their inclination to deep and momentous philosophies, to " a seeking of absolute beauty in the realm of absolute truth."

In summing up the tragic consequences of the German classical movement, which resulted in " one murder, one sudden death, two cases of insanity, another of megalomania," out of the ten writers selected for this book, Miss Butler tries to show that these reactions on a philosophical ideology are typically German. " In what other country would the discovery of serenity, simplicity, and nobility in art have brought about such dire results ? " she asks somewhat accusingly.

The tendency of the German to indulge in ideas and ideologies is indeed

a fact: but are we to take it for granted as Miss Butler does, or should we not do better to analyse its historical causes? One main cause is the unhappy fate of the educated German middle class in the last few centuries. Kept for so long from political responsibility, excluded from the circulation of the political *élites*, this class always preferred to obey and to philosophize. Therefore the tragic political history of Germany, to which Miss Butler refers in the introduction, is not so much, as she thinks, an effect as a cause of that "dangerous idealism" which bore striking fruit in speculation and poetry, but which was surprisingly unable to assist in the formation of a reasonable society.

ERNEST KOHN-BRAMSTEDT.

RUSSIAN SOCIOLOGY. Julius F. Hecker. *Chapman & Hall.* London, 1934. Pp. xvi + 313.

In this useful contribution to the history of sociological theories Dr. Hecker has rightly devoted much attention to their social and political background. For to a greater extent than anywhere else sociology in Russia developed in response to concrete social problems without some knowledge of which many of the theoretical controversies—between Slavophils and Westernists, for instance—become quite incomprehensible. It must also be remembered that very few of the Russian sociologists were professors, to say nothing of arm-chair professors. They were mostly party leaders, social revolutionaries, publicists and journalists. They were concerned with immediate and practical issues rather than with metaphysical subtleties. And if for that reason Russian sociology often lacks profundity it is at the same time free from the obscurity of much continental sociology. Russian sociologists were more interested in changing society than in contemplating it. They regarded sociology as an instrument for guiding and controlling social life—a view that finds its clearest expression in the communist sociology of present-day Russia.

That practical issues predominated is evident when we consider some of the main questions Russian sociologists were confronted with. Should Russia remain an agricultural and therefore religious and conservative society, or should it become industrial and dynamic? Is there a specific Russian civilization, or is it Russia's destiny to synthesize the cultures of East and West? Is the abolition of slavery desirable? Is Russification a wise policy and what is true nationalism? Which class should wield social power and what institutions will best serve human needs? Is it possible to plan society? Even the more theoretical discussions such as those on the nature of social solidarity, the individual versus society, the meaning of progress, the philosophy of history, are never far removed

from concrete realities, nor do they possess that abstract air of being unreal.

Dr. Hecker divides his book into four parts. In the first he deals with Danilevsky, the chief exponent of Slavophilism, with the Russophils—Leontiev and Pobiedonostsev, and with the Westernists, Belinsky, Herzen, Granovsky and Tschernischevsky. The second part—the most satisfactory in the book—considers the Russian subjectivists, Lavrov, Mikhailovsky and Karaev, all of whom exercised considerable influence on the Russian intelligentsia. The third part is devoted to a group of diverse thinkers—Kropotkin, Kovalevsky, Novicov, De Roberty, and Sorokin. The final section analyses communist sociology and the theories of Plekhanov, Struve, Tugan-Baranovsky, and Bogdanov.

Dr. Hecker's study, the first draft of which in 1916 passed unnoticed in England, comes at an opportune time. Sociology, if theoretically the broadest and most generalizing of the social sciences, is characterized by a remarkable insularity of outlook. In each country it pursues almost a separate path and rarely troubles to become acquainted with the problems and achievements of sociology in other countries. Thus many of the good ideas of Russian sociology remained foreign to sociologists generally, although they were later worked out independently and more systematically in Europe. One might with some truth assert that there are Russian, French, German, Italian, and English sociologies, but no sociology as such. It is to be hoped that growing intellectual co-operation among sociologists and the present crisis and clash of ideas will do much to develop a world sociology.

J. RUMNEY.

CIVILIZATION AND THE GROWTH OF LAW. A Study of the Relations between Men's Ideas about the Universe and the Institutions of Law and Government. By William A. Robson. *Macmillan*, 1935.

Dr. Robson has good reason for his long sub-title; for the name by which his book will generally be known begs many questions, not the least being, What do we mean by "civilization"? and What do we mean by "law"? It is, in fact, these very questions, especially the second, which Dr. Robson sets out to answer in his book; and though he is not, technically, a philologist, much of his work is devoted to clearing up the obscurities of language which make the study of his subject so full of pit-falls.

No better example of this important fact need be sought than a well-known modern and interesting work, Dr. Malinowski's *Crime and Custom*

in Savage Society,[1] of which Dr. Robson makes considerable use in his book. As is well known, Dr. Malinowski's monograph is concerned, mainly, to show that there exists among his Trobrianders a system of jurisprudence which fulfils the functions performed by the legal systems of more highly developed communities, even to the extent of recognizing such a technical distinction as that between criminal and civil law—a distinction with which all lawyers, but comparatively few laymen, are familiar. Now the first criticism which a lawyer is inclined to pass on Dr. Malinowski's book is, that his " savages " are not, at any rate for the purposes of historical jurisprudence, savages or primitives at all, but people very considerably advanced from the Stone-Age type to be found, for example, at the present day in Australia, and, till comparatively recently, in Tasmania and Central Africa. As Dr. Malinowski himself tells us in his book (p. 17), the Trobrianders are " a dense population mainly engaged in agriculture and fishing, but experts also in various arts and crafts and keen on trade and exchange "—in other words, a people in an early stage of what, but for the fact that they still retain formal vestiges of a matrilineal system, would be called patriarchalism. Again, though a lawyer cannot expect that an anthropologist, who appears mainly concerned in his work to refute the heresies of other anthropologists, should be tender towards the susceptibilities of lawyers, he ought to be more precise in defining the distinction between criminal and civil law, a distinction of a purely legal character. Apparently, Dr Malinowski finds it in the prohibitory character of the former as contrasted with the constructive nature of the latter. But no lawyer familiar with modern systems of civil law would admit for a moment that such systems do not impose extensive prohibitions; and when the author speaks of breaches of law which are " penalized but not punished " (p. 58), a lawyer can only wonder what subtle distinction marks the difference between a penalty and a punishment.

It is with difficulties such as these that Dr. Robson has had to struggle in his study of the relations between men's ideas about the universe and the institutions of law and government. For he has found, as he has proceeded with his task, that he has to deal, not only with the educated " man in the street", or the philosopher who takes all knowledge for his province, but with several highly organized and, in some cases, pugnacious groups of specialists, all of whom are concerned with the " growth of law," and each of whom adopts a somewhat possessive attitude towards a concept which may fairly be described as an element in the mental make-up of every intelligent person. Very wisely, Dr. Robson has, for the most part, adopted an attitude of enquiry, rather than of criticism ; though he has not attempted to conceal his indignation at some of the worst lapses of

[1] Kegan Paul, 1926.

humanity, as, for instance, when the disappearance of ordeals and other "superstitious uses," from medieval legal procedure, was followed in continental Europe by the far less humane system of official torture.

Whilst he shows an intelligent interest in other aspects of the concept of law, Dr. Robson, as a lawyer of distinction, quite naturally devotes most of his work to that kind of law which attempts to control, or at least to describe, the conduct of human beings living together in societies organized for common action through institutions of greater or less complexity. He deals first with the origins and historical development of legal systems, but quite properly deprecates the assumption, too frequently made, that to understand the origin of a rule of law is to justify its existence, an obvious fallacy which has, in fact, had an all too persistent life, and which has done much to obscure the importance of law as a sociological factor. He dwells then on the closely related topic of the forms which lawyers' law may assume, and offers sound suggestions as to the relative value of each. If he comes down finally in favour of the statute, the statement of law avowedly creative and claiming literal obedience, it is not because he is blind to the many technical dangers of this form of law, but because he regards it as the speediest and most emphatic way in which a community can assert its self-respect as a body of individuals striving to oppose free will to fate.

Finally, in this part of his book, Dr. Robson performs an interesting *post-mortem* operation, by tracing the relics, in a modern system of law, of the influences of magic, superstition, and other survivals which have ceased to have any relation to the existing environment. Here he displays a wide acquaintance with medieval and modern authorities, though he may have relied too implicitly on works such as H. C. Lea's *Superstition and Force*,[1] which, albeit excellent in their day, have been superseded by the researches of more modern scholars; and his inferences from original authorities seem to us, in some cases at least, hardly justifiable.[2] Dr. Robson's illustrations from the history of existing legal systems seem, in fact, to be the weakest part of his book. His account of the Constitutions of Clarendon (p. 134) appears not to be borne out by the text of that

[1] Published in Philadelphia in 1878. Lea's blind acceptance of the whimsies of the compiler of the *Mirror of Justices*, a medieval English work, is a striking example. These mysteries were exposed in masterly fashion by the late F. W. Maitland, more than forty years ago.

[2] For instance, in the change which took place in the interpretation of the cold-water ordeal. Originally, the accused who sank was held guilty; later, sinking was a proof of innocence. Dr. Robson suggests that the change was due to the influence of Christianity, with its insistence on the blessedness of life beyond death. It is an equally plausible view that, as trial by ordeal actually became confined, in its later days, to persons against whom there was a strong presumption of guilt, it was decided to make escape as difficult as possible.

famous document ; and, in his instances of the survival, in comparatively modern times, of the influence of magic in legal procedure, he omits to notice the very suggestive and, at one time, extremely important procedure in " distress," which is directly in line with the Oriental " sitting dharna " and other spells.

Having disposed of the origin and development of legal systems, Dr. Robson turns, in the second Part of his book, to fulfil the promise of his sub-title by relating his juristic enquiries to the general attitude of philosophers and speculators towards the universe as a whole, summed up under the well-known phrase " Law of Nature." Perhaps, as a jurist, he is inclined to over-estimate the influence of legal thought on the minds of the exponents of this famous body of doctrine, the contents of which have varied from age to age, and from writer to writer ; and his charge of plagiarism levied against Dr. Karl Pearson, Sir Arthur Eddington, Sir James Jeans, and other modern exponents of the philosophy of physical science (pp. 295–6), seems to be a little far-fetched. But Dr. Robson's account of the various forms assumed by the doctrines of the Law of Nature, and the practical conclusions drawn from them, is admirably clear ; and, in his handling of this difficult subject, his happiness of phrase and lucidity of reasoning are seen at their best.

In the third and last Part of his book, Dr. Robson returns to a fuller examination of the brief prologue on the Nature of Law, and the scope of its operations, with which the book opens. This course he adopts in pursuance of his avowed object, the building of a synthesis which shall reconcile and harmonize the conclusions produced by a long period of perhaps excessive specialization. In this he is in accordance with the modern spirit, which is wearied and depressed by the apparently irreconcilable quarrels of rival seekers after truth ; and he will surely have the sympathy of students of sociology, which is itself a synthesis of many sciences. In his quest, he turns for hope to the new forms of harmony which the growth of international intercourse is producing, and which may, in the future, give birth to a new concept of Law under which all hitherto rival specialisms may take refuge. Such a suggestion has already been preshadowed by the eminent French thinker, Léon Duguit, with whose views Dr. Robson does not, apparently, deal. But there is plenty of room for new workers in the field ; for it will be hard to overcome the age-long traditions of professional prejudice and scientific training. And, indeed, it needs an effort of the imagination to conceive of a theory of Law which shall be elastic enough to cover alike the organic and the inorganic world, and, in the former, such widely differing spheres as human, animal, and vegetable life.

In his modest Introduction, Dr. Robson apologizes for his boldness in

attempting a task which might well daunt the boldest of explorers. His apology is entirely unnecessary. He has set before himself an ideal with which all who have at heart the progress of the human spirit will sympathize, and towards which the noblest minds in all branches of learning are striving. If he has not completely satisfied either himself or his readers, he has at least emphasized the need for further progress, and stimulated them to further efforts. To travel is often better than to arrive.

EDWARD JENKS.

DYNAMIC SOCIAL RESEARCH: By John J. Hader and Eduard C. Lindeman. *Kegan Paul*, 1933, 12*s.* 6*d.*

Methodological discussions can be the most sterile preoccupation of sociology. The empirical specialist is often justified in turning with contempt from what appears to be futile hair-splitting about words or pompous regurgitations of platitudes by logicians who have never grappled with the difficulties of concrete social research. But if the *a priori* deductive methods of some German sociologists seem to lead nowhere, it is equally certain that the infatuation for numbers and "facts" of some American empiricists is a blind alley. In the natural sciences the advance of knowledge has been due to the parallel development, in mutual dependence, of experimental technique in the laboratory and conceptual methods of analysis and interpretation. In sociology it is still permissible to elaborate theories and concepts without consideration of empirical discoveries, which are, in their turn, too often undertaken without reference to current theory.

Dynamic Social Research is a welcome attempt to discuss methods and concepts in relation to the results and problems of a limited field of empirical research. It is a "project in research method rather than a conventional fact-finding inquiry," say the authors. They set out to investigate the workings of joint committees of employers and employees in industry, not as a circumscribed economic "fact," but as a social phenomenon. Significantly, they reject what they describe as "spectator" knowledge claiming to be free from a valuational bias, and urge that social science is concerned not only with discovering facts, but with "implementing social change." They refuse to isolate their problem, but insist that it should be investigated in "the context of the larger whole of socio-economic society."

It is an ambitious approach; and the authors can hardly be blamed if their achievement falls short of the anticipations they arouse. They lapse into an awkward jargon at times, as when they declare that "confronting a social problem represents an act of wholeness before it can become significant as detail"; and in their zeal for the larger issues they

dogmatize about matters which are not beyond controversy. What, for instance, are the grounds for believing that " antithesis, conflict, cannot be eliminated without reducing life to helpless monotony and sterility ? "

The joint committee is but a fragment of American industrial organization. Sociologically, it could be regarded in either of two ways. It could be investigated as one phase in the enormous range of contact and interaction between employers and employees, management and workers, labour and capital. This would involve a consideration of the whole structure of American industry in relation to the economics and economic history of American capitalism and to law and public administration. The problem is propounded by Hader and Lindeman; but they skirt it with generalities which leave it in the air and fail to convince the reader that the joint committee is anything but a device to combat trade-unionism.

Alternatively, the joint committee itself might be made the focus of inquiry; not in its significance as a phase of employer-employee relationships, but in regard to its internal constitution and dynamics as a typical institution of American industrial life. In so far as this problem is their main concern Hader and Lindeman have made a useful contribution to the clarification of research method in sociology.

They begin by positing four " categories of analysis," concepts by the aid of which the processes of joint committee activity may be analysed and interpreted. These " psycho-sociological " categories they name (*a*) *Impulsion*, defined as " . . . the needs, wants, desires and purposes generated by individual human beings but expressing themselves in social or collective modes "; (*b*) *Circumjacence* ". . . those elements in the total situation which condition, limit, channel the social group under observation . . ." including under this its structure, the " personal equations " of the participants and their " social philosophies "; (*c*) *Interaction*—" the quantity and quality of participation in the committee process "; (*d*) *Emergence*—" any evolutionary change in the quality of the consequences of joint committee action observable in the committee's impulsion, circumjacence, and interaction."

These concepts are deductively derived and carefully, if somewhat unskilfully expounded. The authors have not provided sufficient observational material to prove that their four categories are either all necessary or exhaustive, even with respect to their particular problem. The value of these concepts lies in that they represent a *method* which must commend itself to anyone engaged in similar research. American sociology has been mainly responsible for this method of what might be called situation analysis. It has the merit of modesty and the advantage of being applicable to the realities of social life. By limiting its unit of investigation to

something tangible, visible and susceptible of detailed observation, it avoids the danger of intuitive generalizations which has made sociology the wordy battle-ground of rival metaphysicians. It is a method which provides the research worker engaged in the direct study of social groups and social behaviour with a starting point which he could never find in the lifeless abstractions of a Wiese, the metaphysical hypostatizations of a Vierkandt, or the sweeping constructions of a Durkheim. In social anthropology, the present reviewer's field of research, such methods are receiving increasing favour.

An appropriate conceptual apparatus is however only half the equipment necessary for conducting social research. Just as the natural scientist has to know how to carry out experiments in the laboratory as well as how to interpret and think about them, so in social science we require techniques of research as well as concepts. This, as anyone embarking upon a concrete investigation soon discovers, is usually accorded an airy indifference by otherwise voluminous systematic sociologists.

In the concluding chapters of *Dynamic Social Research* the authors dicuss the " techniques and devices " employed by them. It is the best portion of the book, explicit, well documented with charts and synoptic tables, and thorough. Six techniques—interviewing, participant observing, observing, case analysis, charting and statistics—are described in detail. It is impossible to summarize this section of the book here; but it deserves the earnest attention of every student engaged in field study of group life.

Hader and Lindeman's book pertains to a restricted province of social science. But their research methods have a sufficiently wide applicability to mark their book as a valuable contribution to current sociology.

M. FORTES.

A HUNDRED YEARS OF ANTHROPOLOGY. By T. K. Penniman. *Duckworth*, 1935. 15*s*.

To write the history of such a complex and badly integrated study as the Science of Man is bound to be a difficult task, and it says much for Mr. Penniman's courage that he has attempted it, and for his ability that he has succeeded so well. From the immense mass of material, much of it of indifferent worth, that confronted him, he has made a systematic arrangement that has at least the merit of simplicity. Following the suggestion of Dr. Marett in a lecture before the International Congress of Anthropology last year he has divided his field into four periods—the formulary, before 1835; the convergent, from then till 1859; the constructive, from 1859 to 1900; and the critical period, from 1900 till the

present day. The selection of the first date is presumably dictated by the title of the book; that of 1859 and of 1900—of the publication of the *Origin of Species* and of the rediscovery of Mendel's work respectively—indicates the importance which the author attaches to the bearing of biological studies upon anthropology. One of his principal theses is the unity of the sciences which deal with various branches of man's activities, and in this respect his work will be very useful in making the specialist student aware of what has been done in fields allied to his own.

The author insists also that the field of investigation should take in civilized as well as primitive peoples. "Let us understand," he says, "that Anthropology is not merely barbarology, but the whole science of Man." The difficulty of such a claim is that it needs a superman to make good its implications. We find, in fact, in this book, no discussion in the literature of the economics of modern civilized societies, or of their history (e.g. Arnold Toynbee's recent work is not mentioned), and little reference to sociological theory apart from Durkheim and Sorokin. However, the author should not be treated too harshly on having omitted to fill out in its entirety the nominal range of his subject matter.

Where he is open to criticism is in respect of the lacunae in the more strictly defined primitive field, particularly in the present-day period. One is unable to find, for example, any reference to the work of either Kroeber or Lowie, whose books for the past decade have provided students with some of the few essays in that general comparative sphere for which the author pleads so hard. There is also no mention of Te Rangi Hiroa (Peter Buck), one of the best living technologists, whose series of volumes on Polynesian material culture are a recognized model of method. The analysis of recent work in linguistics is decidedly inadequate, two pages being devoted to the theories of E. J. Payne, and one to that of Paget, while the contributions of Sapir, Jespersen, Meillet and Gardiner (let alone I. A. Richards and Malinowski) are totally ignored. A well-deserved tribute is paid to Sir James Frazer, but one would gather from this book that work on Australian totemism had ceased with Baldwin Spencer. And while the author includes some interesting observations of his own on the four-class and eight-class system he does not give even a line to the work of Elkin, of Lloyd Warner, and above all of Radcliffe-Brown, who during the past ten years by steady field work and theoretical analysis have revolutionized our knowledge of these kinship groups. Again, the importance of the changes that have taken place in methods of anthropological field work in the last decade has apparently passed almost unnoticed in a valiant attempt to weigh and reconcile the claims of the various theoretical "schools." In the list of Museums, Societies, and Periodicals at the end of the book there are some notable omissions—

e.g. the University of California Publications in Archaeology and Anthropology, the Memoirs and Bulletins of the Bernice Pauahi Bishop Museum of Honolulu, *Oceania* (medium of publication of the immense amount of work done in recent years under the auspices of the Australian National Research Council) and even *L'Année Sociologique*!

In view of the recent publication of a second edition of Haddon's *History of Anthropology* a book on what is virtually the same subject should present either a great accretion of new matter or an entirely fresh point of view. The present work can hardly be said to do this, and it is a pity that the author's undoubted industry and powers of co-ordination could not have had fuller scope in the treatment of a single section of his subject.

RAYMOND FIRTH.

BOOKS RECEIVED

Allen and Bowley. FAMILY EXPENDITURE. King. 9*s*.
Bastide (R.). ELÉMENTS DE SOCIOLOGIE RELIGIEUSE. Colin. 10 Fr. 50.
Bready (J. W.). DR. BARNADO. Allen & Unwin. 2*s*. 6*d*.
Browne, Ludovici, Roberts. ABORTION. Allen & Unwin. 4*s*. 6*d*.
Butler (E. M.). THE TYRANNY OF GREECE OVER GERMANY. C.U.P. 15*s*.
Craven-Ellis (W.). THE REBUILDING OF BRITAIN. Allen & Unwin. 2*s*. 6*d*.
Dawson (C.). RELIGION AND THE MODERN STATE. Sheed & Ward. 6*s*.
Emmerson and Lascelles. GUIDE TO THE UNEMPLOYMENT INSURANCE ACTS. Longmans, Green. 6*s*.
Fisher (A. G. B.). THE CLASH OF PROGRESS AND SECURITY. Macmillan. 8*s*. 6*d*.
Ginsberg (M.). THE UNITY OF MANKIND. O.U.P. 2*s*.
Goldsmith (M.). SEVEN WOMEN AGAINST THE WORLD. Methuen. 10*s*. 6*d*.
Grierson (H. J. C.), Editor: THE LETTERS OF SIR WALTER SCOTT, 1825–1826. Constable. 18*s*.
Groves (E.). UNDERSTANDING YOURSELF. Allen & Unwin. 7*s*. 6*d*.
Hankins (F. H.). INTRODUCTION TO THE STUDY OF SOCIETY. Macmillan. 17*s*.
Harris (S. H.). THE DOCTRINE OF PERSONAL RIGHT. Barcelona. 10*s*. 6*d*.
Huxley and Haddon. WE EUROPEANS. Cape. 8*s*. 6*d*.
Kantorowicz (H.). DICTATORSHIPS. Heffer. 2*s*.
Kloeren (M.). SPORT UND REKORD. Tauchnitz. RM. 15.
Lambert (R. S.). WHEN JUSTICE FALTERED. Methuen. 10*s*. 6*d*.
Leyburn (J. G.). FRONTIER FOLKWAYS. Yale. $3.00.
Löwe (A.). ECONOMICS AND SOCIOLOGY. Allen & Unwin. 5*s*.
McKee (S.). LABOR IN COLONIAL NEW YORK, 1664–1776. King. $3.00.
Marett (J. R. de la H.). RACE, SEX, AND ENVIRONMENT. Hutchinson. 21*s*.
Marsh (L. C.). EMPLOYMENT RESEARCH. O.U.P. 10*s*. 6*d*.
Marvin (F. S.). OLD AND NEW THOUGHTS ON THE MODERN STUDY OF HISTORY. Nicholson & Watson. 4*s*. 6*d*.
Muirhead (J. F.). LAND AND UNEMPLOYMENT. O.U.P. 7*s*. 6*d*.
Oakes (C. G.). SIR SAMUEL ROMILLY. Allen & Unwin. 18*s*.
Orton (J. L.). EMILE COUÉ: THE MAN AND HIS WORK. Mott. 8*s*. 6*d*.]
Penniman (T. K.). A HUNDRED YEARS OF ANTHROPOLOGY. Duckworth. 15*s*.
Plimmer (U. G.). FOOD VALUES AT A GLANCE. Nicholson & Watson. 4*s*. 6*d*.
Proesler (H.). DIE ANFÄNGE DER GESELLSCHAFTSLEHRE. Erlangen.
Pundt (A. G.). ARNDT AND THE NATIONALIST AWAKENING IN GERMANY. King.
Schöffler (H.). ENGLAND DAS LAND DES SPORTES. Tauchnitz. RM. 2.
Solmes (A.). THE ENGLISH POLICEMAN, 1871–1935. Allen & Unwin. 5*s*.
Studebaker (J. W.). THE AMERICAN WAY. McGraw Hill.
Sturzo (L.). ESSAI DE SOCIOLOGIE. Bloud & Gay. 20 Fr.
Tönnies (F.). GEIST DER NEUZEIT. Buske. RM. 6.
Tower (J. A.). THE OASIS OF DAMASCUS. Beirut.
White (F. W.). BIRTH CONTROL AND ITS OPPONENTS. Danielsson. 3*s*. 6*d*.
Wickwar (W. H.). BARON D'HOLBACH. Allen & Unwin. 7*s*. 6*d*.
Ziegenfuss (W.). Versuch über das Wesen der Gesellschaft. Buske. RM. 3.

A CENTURY OF MUNICIPAL PROGRESS, 1835–1935. Allen & Unwin. 21*s*.
EDUCATION FOR CITIZENSHIP IN SECONDARY SCHOOLS. O.U.P. 4*s*. 6*d*.
MÉLANGES OFFERTS Á ERNEST MAHAIM PAR SES COLLÈGUES, SES AMIS, SES ÉLÈVES. 2 vols.
DOWN BUT NOT OUT. Allen & Unwin. 5*s*.
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AMERICAN JOURNAL OF SOCIOLOGY . . . . . Nov.
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ANNALS OF THE AMERICAN ACADEMY . . . . . Nov.

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Arhiva	Vol. XII., 3-4
Asiatic Review	Oct.
Character and Personality	March, Sept.
Charity Organisation Quarterly	Oct.
Council for the Preservation of Rural England	Dec.
Economic Journal	Dec.
Economica	Nov., Dec.
Études Sociales	Oct., Nov.
Eugenics Review	Oct.
Geographical Review	Oct.
Geography	Sept., Dec.
Headway	Oct., Nov., Dec.
Highway	Nov., Dec.
Hindustan Review	Oct.
International Conciliation	Oct., Nov.
International Labour Review	Sept., Oct., Nov.
International Review of Agriculture	Aug., Sept., Oct., Nov.
Journal of Commons, Open Spaces, and Footpaths	Oct.
Journal of Heredity	Sept., Oct., Nov.
Journal of Negro History	Oct.
Journal of the London Society	Oct., Nov., Dec.
Kent Education Gazette	Oct., Nov., Dec.
Kyoto University Economic Review	July
Man	Oct., Nov., Dec.
Marriage Hygiene	Aug.
Millgate Monthly	Oct., Nov., Dec.
Musée Social	Sept., Oct., Nov., Dec.
New Era	July–Aug.
Planning	Oct.
Plus Loin	126, 127, 128
Population	Nov.
Quarterly Journal of Economics	Nov.
Religions	Oct.
Revue de l'Institut de Sociologie	July-Sept.
Revue de l'Université de Bruxelles	May, June-July
Revue des Sociologistes	July, Aug., Sept.
Revue Internationale de Sociologie	Sept.-Oct.
Rivista Internazionale di Scienze Sociale	Sept.
Rotary Wheel	Oct., Dec.
Sbornik	Vol. X., 4
Scientia	Oct., Nov., Dec.
Scottish Geographical Magazine	Sept., Nov.
Slavonic Review	July
Social Forces	Oct.
Social Research	Nov.
Sociology and Social Research	Sept.-Oct., Nov.-Dec.
South African Journal of Economics	Sept.
Sozial-und-Wirtschaftsgeschichte	Dec.
Statistique Général de la France	July-Sept.
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